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BROADCASTING IN GREAT BRITAIN

IN the White Paper on Broadcasting Policy issued in July 1946, the Government announced its intention to renew the charter and the licence of the British Broadcasting Corporation, with certain modifications, for a period of five years from January 1, 1947. The committee under Lord Beveridge now considering the whole problem of the organisation of broadcasting in Great Britain has therefore little more than a year in which to complete its inquiries and report before a decision regarding renewal of the charter will be imperative. Nor is the matter receiving much public discussion, either in or out of Parliament: the Royal Commission on the Press, reporting last year, was noticeably silent on a factor affecting the freedom of the Press on which comment might well have been expected in view of the strenuous opposition at one time manifested by the Press to the broadcasting of news bulletins by the British Broadcasting Corporation.

Some attention was directed to the problems and responsibilities of broadcasting by the Director-General of the B.B.C., Sir William Haley, in his talk on "The Place of Broadcasting" in connexion with the silver jubilee celebrations of the Corporation in November 1947, and in his subsequent Lewis Fry Memorial Lectures, "The Responsibilities of Broadcasting", at the University of Bristol on May 11 and 12, 1948. Otherwise there has been little public discussion since a series of articles like "The B.B.C. Marks Time" appeared in *The Round Table* of September 1946, which synchronized with the debates in Parliament on the White Paper. Even as an example of a public corporation or from the point of view of the bearing of such a monopoly on staff questions and the problem of the 'closed shop', the B.B.C. has been little considered in the discussions on nationalized industry.

Lord Reith's recent autobiography, "Into the Wind", enables one to appreciate some of the issues at stake, and indeed that book contains valuable contributions to the formation of sound public opinion on the future of British broadcasting. Further material is provided in two other books. In the first of these*, in the Viewpoint Series, the Controller of the Third Programme of the B.B.C. gives his personal views of the relation of broadcasting to society, more specifically in the framework of a Christian concern, and the obligations and implications connoted by an organisation which constitutes a monopoly. The second book† deals more particularly with British broadcasting as a monopoly; it is offered as a specific contribution to reasoned discussion of the subject, and a very valuable feature of the book is the third part, with a lucid and well-balanced and documented summary of public

* Broadcasting and Society: Comments from a Christian Standpoint. By Harman Grisewood. (Viewpoints, No. 15.) Pp. 96. (London: S.C.M. Press, Ltd., 1949.) 2s. 6d. net.

† British Broadcasting: a Study in Monopoly. By R. H. Coase. (Publications of the London School of Economics.) Pp. ix+206. (London, New York and Toronto: Longmans, Green and Co., Ltd., 1950.) 12s. 6d. net.

discussion of British broadcasting since 1926, which is by no means limited to the monopoly aspect.

From that point of view, however, the most striking omission is any consideration of the relation of the Press to broadcasting. It is true that the news bulletins and the broadcasting of running commentaries on important public events are only a small element in the radio programmes. Nevertheless, they have a vital bearing on the question of a free Press and fair comment, and broadcasting and the Press may well constitute a mutual check on abuse, whether from within or under external pressure from government or other influences. In view of Mr. Coase's attention to the attitude of the Press in the early days of broadcasting, it is surprising that he does not examine further the possibility of some such balance of powers acting as a check against abuse in either sphere, particularly in view of the way in which, at the time of the General Strike in 1926, the B.B.C. endeavoured to preserve its tradition of accuracy and fair play, and so advanced in general esteem.

There is a passage in Lord Reith's autobiography which just touches on this question of the relations between the Press and broadcasting, and the possibility that, rightly understood and applied, a national broadcasting service might supply the integrating factor which democracy needs to-day. The idea seems to be implicit in Sir William Haley's lectures also, and in Mr. Grisewood's contribution. The theme should be examined carefully by the committee inquiring into the future organisation of British broadcasting; for the fundamental question whether a Government monopoly of broadcasting is justified can no longer be ignored.

The report of an inquiry into the working of a public corporation with more than a quarter of a century of service behind it cannot fail to be of the widest possible general interest to-day, quite apart from the immediate issue of broadcasting policy. Even Mr. Coase's brief reference to the bearing of monopoly on the staff problem of the B.B.C. should not be overlooked by professional associations generally. It brings out clearly enough the implications for individuals of the 'closed shop' principle, and the possible disadvantages of a monopoly to those engaged in any of the services or industries which by nationalization are converted into monopolies. Furthermore, reading between the lines of Lord Reith's book, it is obvious that there is room for a good deal of discussion as to the exact form the relation of the Corporation to the Government should take on a monopoly basis. The Postmaster General, on the evidence of the past twenty years, has scarcely been the most appropriate minister to carry the responsibility, and transfer of that responsibility to such a minister as the Lord President of the Council might be more in harmony with functional responsibilities, and also avoid the anomaly of a minister being placed occasionally in a position to be judge in his own cause.

Mr. Coase does not attempt to reach any conclusion as to whether or not it is desirable that broadcasting in Great Britain should, or should not, be a monopoly. He sets out to answer the questions how broadcasting in Great Britain came to be organised on a monopolistic basis, what has been the effect of the monopoly on the development of, and policy towards, competitive services such as 'wire' broadcasting and foreign commercial broadcasting for listeners in Great Britain, and what are the views which have been held on the monopoly in Great Britain. His final examination of the cogency and implications of the arguments by which the monopoly has been justified, and the discussion of the forces in society which have contributed to the widespread support which the monopoly has enjoyed, is not without imagination, and in particular he re-directs attention to two important contributions to thought about broadcasting policy which were made in the midst of the debate in July 1946.

The first of these was Sir Ernest Barker's discussion in the *B.B.C. Quarterly* of broadcasting as an active element in democracy. The second was a more practical suggestion in the final report of the New Towns Committee which has passed almost unnoticed. This passage of the report considered the possibility of establishing municipal broadcasting stations to assist in developing a sense of community, and visualized such a service as providing a forum for the free and open discussion of many of the problems which will arise in the course of development of a new town. It could keep the citizens in regular touch with the activities of the agency and the local authority, and could become an effective channel for constructive criticism on every subject.

Such a suggestion has most important implications in the rather vexed question of local government publicity; but in the present connexion it is of particular interest as a corrective to dangers in a government monopoly. The New Towns Committee, of which it will be recalled that Lord Reith was chairman, had in mind the possibility of a national development which would increase the number of available programmes, although it was contemplated that the content of the programmes broadcast would be controlled by a new supervisory body on which the British Broadcasting Corporation would be represented. As such it reflects perhaps the way in which Lord Reith's fairmindedness has led him to seek ever fresh ways of correcting the dangers inherent in a monopoly which he believed was imperative if standards were to be maintained and broadcasting run in the public interest as a public service.

Those deeper issues and the question of standards are discussed more fully in Mr. Grisewood's little book. First, however, it is of interest to note how Lord Reith's views find expression in the talk which Sir William Haley broadcast in "The Place of Broadcasting" in connexion with the silver jubilee celebrations. Regarding broadcasting as a public service run by an independent corporation, he emphasized its responsibility to the disinterested search for truth, and for absolute impartiality in all matters of controversy. Next, used as a means of education in the broadest sense, it involves using the microphone to inform and interest listeners in all things that matter,

to inculcate citizenship, pay proper attention to public affairs, encourage tolerant discussion and widen so far as possible the range of debate. Finally, there is the responsibility of raising standards.

To succeed, all this must be done, Sir William Haley insists, within the limits of the broad contract that the listener must be entertained and that broadcasting is not an end in itself. It must aim at making people active and not passive, both in the fields of recreation and public affairs. Here, it should be noted, Mr. Grisewood points out that the unifying influence of broadcasting seems to be somewhat superficial in its effect and has contributed little towards nourishing the deeply experienced and freely willed unity with a living moral tradition which is the condition for diversity and stability in society. Accordingly, the question of impartiality becomes of special importance, not only because it is an essential quality if a monopoly is to be maintained, but also because it closely affects the diffused pattern of values—scientific as well as ethical—which can still be traced within our society.

It is not sufficient to accept the view that the British Broadcasting Corporation's discharge of its trust in this respect has been consistently scrupulous and thorough. Its ideas of impartiality cannot be at variance with those that are held by the society it serves; nor could the public, as Mr. Grisewood has elsewhere observed, expect to enjoy for long the benefits of impartiality and tolerance, and at the same time permit a condition of society in which these virtues are being extinguished. Fundamentally, of course, that was Lord Vansittart's concern in a recent, but perhaps somewhat injudicious, speech; and these points should also be engaging the thought of all those concerned to maintain the tradition in Great Britain of freedom of investigation and publication. The man of science in particular should heed Mr. Grisewood's warning as to the need for knowing when impartiality should give place to decision.

Though he writes particularly from a Christian point of view, Mr. Grisewood deals with issues which concern the man of science—whether he calls himself Christian or not—and he stresses the importance of the listener's responsibility in securing the right and full use of the powers of broadcasting. The maintenance of standards and the problem of balance in society both demand co-operation on the part of the listener, and Mr. Grisewood's chapter on society's responsibility for broadcasting should help to direct increased attention to the process rather than to the material that is broadcast, and the dependence of the latter upon the vitality and diversity of the society which broadcasting serves. Here he points the way to some fundamental thinking about the use of broadcasting for educational purposes which is of particular interest, and he emphasizes his point by quoting from one of Sir William Haley's Lewis Fry Memorial Lectures: "Broadcasting, without its responsibilities, is nothing. It is not a way of thought; it is not a way of culture; it is not a way of life. It is there to serve thought so that people shall think for themselves. It is there to serve culture in such a way that people will turn more and more to active participation in the arts . . . and help to build a community in which wireless is only a very small part of a full and satisfying life."

Mr. Grisewood does not attempt to give answers to the questions he raises, but rather to stimulate the thinking which will contribute to the solution of the problems created by the impact of broadcasting and other factors upon our society. He shatters the complacency with which we are apt to regard such ideas as impartiality and toleration, and recalls us to the earlier view of Bagehot that tolerance is a condition of society, deserved and earned by society, the exercise of which carries responsibilities not only for those charged with holding the balance but also for society as a whole. More constructively than Middleton Murry, he discusses the problem of tolerance of the intolerant, and he challenges, too, our practice of using language regarding human rights and freedoms without knowledge and understanding of what sanctions uphold them, and without regard to the severe and profound philosophy, the spiritual discipline and profession of faith from which alone these rights and freedoms still draw their sustenance.

Throughout these two books and Sir William Haley's lectures runs the consciousness of how much Lord Reith has done to establish British broadcasting as an independent institution with a high tradition of public service and conception of moral purpose. Equally they make plain how much the continuance of that tradition and the fulfilment of those ideals, whether under the continuance of a monopoly or not, depend on the understanding and co-operation of the public that broadcasting serves. They indicate, too, that the creative thought and sound judgment and decision which are involved affect vitally the course and development of science itself, and they invite the co-operation of the man of science as such, as well as citizen, in shaping an instrument which will serve our purposes even more effectively.

RICHARD WILLSTÄTTER

Aus meinem Leben

Von Arbeit, Musse und Freunden. Von Richard Willstätter. Herausgegeben und mit einem Nachwort versehen von Arthur Stoll. Pp. vii+453+33 plates. (Weinheim/Bergstrasse: Verlag Chemie G.m.b.H., 1949.) 28 D. marks.

THIS is a record of achievement, fame, suffering and frustration, written by a great chemist and teacher towards the end of his eventful life, when he could view acts and facts in perspective and draw conclusions with judicial fairness and his wonted scientific precision. It differs from orthodox biographies of learned men by the emphasis which is laid on the human factors in explanation of their attainments and shortcomings in various branches of science. The book goes far beyond a chronicle of events in Willstätter's life; it is a valuable contribution to the history of chemistry and a thinker's indictment of the political currents in Germany between the two World Wars, and their influence on the state—and degradation—of science and academic freedom.

The author begins by tracing, with undisguised pride, his ancestry back two centuries. This family chronicle, which, incidentally, is an illuminating contribution to the cultural history of German Jewry, provides the background for his moral and intellectual development and helps us to understand the emotions released by the excesses of Hitlerism in the outlook of this high-minded, just and intensely patriotic man. He proceeds to speak of his life from childhood, through school and university, and of his early academic career at Munich. The story of his courtship of Sophie Leser and their married life proves his capacity of integrating into one organic whole, fond devotion to his wife and two children with the untiring pursuit of his scientific aims and his deep sense of duty to his students. This period of complete happiness ended after five short years when the young wife and mother died after an operation for appendicitis, unduly delayed by the surgeon "who on principle did not operate at night". This was the first tragic blow in Willstätter's life. He endeavoured to sustain it by taking the mother's place in the care of his children and by further intensifying his work.

The following chapters deal with his tenure of the chair of chemistry at Zurich and the directorship of the Kaiser Wilhelm Institute for Chemistry at Dahlem, periods largely devoted to the study of chlorophyll and anthocyanins. He interposes "Memories of Adolf von Baeyer", Liebig's successor and his own predecessor in the Munich chair. This appreciation of his master's character, life and work, dictated by "gratitude, admiration, reverence and love", also serves as a critical survey of the progress of organic chemistry during a century which began with the foundation of the first teaching laboratory—at Giessen—by Liebig and continued down a direct succession of professors and students, Kekulé, Baeyer, Willstätter—a truly noble scientific lineage.

The chapter in memory of Willstätter's intimate friend Haber shows the sharp contrast between the personalities and the methods of approach to scientific problems of the two men. It records, *inter alia*, Haber's work on war gases in 1915 and Willstätter's own part on the protective side, namely, the use of hexa-methylene-tetramine as a charge in gas masks.

During the Dahlem period Willstätter suffered another irreparable loss: his son, ten years old, died suddenly of previously undiagnosed diabetes. Little wonder that after this second blow Willstätter, who himself had made fundamental contributions to biochemistry, for example, his work on enzymes, and had furnished medical science with valuable new drugs, lost faith in the profession—hence his bitter remark on the passing of a colleague: "He died of his doctor."

In 1915 Adolf von Baeyer decided to retire and proposed Willstätter, his most outstanding pupil, as professor at Munich. The latter, who was then considering calls from several other universities, felt it his duty to accept. The ten years of his occupation of the Munich chair proved to be the most eventful of his career. Enormous difficulties had to be faced under war conditions in modernizing the laboratory, which had remained practically unchanged since Liebig's time, and in re-organising the teaching staff to satisfy the demands of the new chemistry. After the armistice in 1918 students flocked to the University, overcrowding lecture theatres, and courses had to be given in repeated shifts. Soon the storm of revolution broke loose and the Republic of Councils was set up. The chapter dealing with this exciting period, written with Willstätter's keen sense of observation and calm discrimination, should appeal to a wider circle than the chemical fraternity in explanation of the social and political trends in Germany which eventually led to the seizure of power by the Nazis.

On accepting the Munich professorship, Willstätter decided to give to teaching and to the creation of a 'school' priority over his own investigations; nevertheless, he contrived to carry out in these ten years researches of the highest order on enzymes, hydrogenation with oxygenated platinum and sodium amalgam and with fermenting yeast. He applied the yeast method with conspicuous success to the preparation of tri-bromalcohol. This compound proved to have valuable narcotic properties, and he was sorely disappointed in his days of need by the shabby treatment he received from the concern which took up its commercial exploitation.

Willstätter's achievements were recognized by academic honours, including the Nobel Prize, and high decorations in many countries; but all the distinctions that were showered on him could not dispel his apprehension of the repercussions of political trends upon the life of the University. After the Hitler *Putsch* in November 1923 the threat to academic freedom took concrete form, when appointments of non-Aryans to vacant professorships had to be considered. Willstätter's objections in two cases were overruled because "one had to have regard for the time and the street". When a meeting of the faculty decided on racial grounds against inviting Victor M. Goldschmidt of Oslo to follow Paul Groth in the chair of mineralogy, Willstätter refused to be a party to this violation of academic principles, and within a few hours handed in his resignation.

He thus voluntarily ended his brilliant academic career, but by no means his service to science. Although deprived of experimental facilities—he never again set foot in the University laboratory—he directed work by devoted former students. He now found time for writing up older investigations. His fixity of purpose is shown by researches on enzymes carried out under incredibly difficult conditions with the assistance of Dr. Margarete Rohdewald, who nightly reported by telephone the laboratory work of each day, the discussions and calculations being in later years often punctuated by the click of the Gestapo's tapping instrument.

The period of self-imposed leisure afforded time for travel, lectures in England, America and elsewhere, of which he regarded his Faraday Lecture to the Chemical Society in London in 1927 as the most important. It also allowed him to satisfy his artistic bent by acquiring a notable collection of pictures, particularly of the French school.

Willstätter, as one of the earliest victims of the Nazi movement, must have been fully aware of what awaited him when Hitler came to power in 1933. But he stubbornly resisted any thought of emigration, until in November 1938 he narrowly escaped the concentration camp by a subterfuge of his trusted housekeeper. During the following months, when he was dispossessed of his house and most of his library, pictures and chattels while being subjected to unbelievable indignities and pinpricks, he maintained his philosophical calm until he could endure the strain no longer. Without help or preparation he made an abortive attempt at escape by rowing-boat

across Lake Constance. After two days in prison he at last obtained his permit of emigration, and on March 4, 1939, was received by friends in Switzerland. With Aristides, he felt it hardest that his fatherland had dishonoured itself by banning him.

The book was begun soon after he settled at "Eremitagio" in Muralto-Locarno, finished in December 1940 and was intended to be further revised. Those who knew him will recognize by style and substance a life-like picture of the author and his ways. The record of many events in his life takes the form of anecdotes; they are not pointless reminiscences of an old man in his 'anecdotage', but are as incisive as his methods of research. While tempered by a genuine sense of humour, they mostly have a sting in their tail. Although fully aware that few of those mentioned would be pleased, he is scrupulously fair to all, not least to those who have done him wrong. He dedicated the book "to his devoted pupils", who owed more to him than he to them. He pays generous tribute to others, for example, to Sir Robert and Lady Robinson for continuing his own work on anthocyanins by their masterly investigations and syntheses of natural glucosides.

The author did not "wish to write a chemical book, nor to digress too far from the life of a chemist". His success in both directions is exemplified by his inability to dissociate even the most intimate domestic events from his work; thus, he fixes the birth of his daughter by his work on complex magnesium in chlorophyll, and the death of his wife by that on the colourless orthoquinone. This interweaving of life and science throughout the book makes it an unusually interesting document of the work and personality of one of the greatest figures of his generation in the world of chemistry.

The author's own story ends with his final departure from the country of his birth. It is taken up in an epilogue by the editor, Prof. Arthur Stoll, his favourite pupil and former assistant, who befriended him in his exile. Prof. Stoll, with a devotion for which all Willstätter's friends will be grateful, records letters and conversations during the last three years of his master's life, in which he discusses the progress of the autobiography, the publication of his last researches and new proposals for the technical preparation of certain drugs. In the quietude of his newly found Swiss home and garden, Willstätter shed all bitterness and regained his innate serenity of mind. He worked uninterruptedly until in December 1941 his weakened heart began to give him trouble. He died on August 3, 1942, ten days before his seventieth birthday.

Credit is due to the publishers for the excellent production of the book and the quality of the illustrations, some of which are of historical interest.

R. LESSING

SOCIAL OR COMMUNITY MEDICINE

Recent Advances in Social Medicine

By Prof. Alan Carruth Stevenson. Pp. viii+241. (London: J. and A. Churchill, Ltd., 1950) 18*s*. net.

SINCE social medicine has become a familiar expression in Great Britain, there has been a great deal of discussion about its meaning, and no general agreement has been reached. Prof. A. C. Stevenson, in his book in the "Recent Advances" series, wisely refrains from flogging a dead horse by adding to the controversy. Whether social medicine is regarded as an academic discipline in its own right or as a particular way of viewing a wide range of medical problems, it is clear that the subject has a considerable statistical content, if only for the reason that, however regarded, social medicine interests itself mainly in health and disease in the mass. It is not surprising, therefore, that a good half of the 241 pages of this book is taken up mainly by medical and social statistics and relevant accounts of statistical methods. Chapter 13, written by Dr. E. A. Cheeseman, is particularly noteworthy in this respect. This chapter includes a simple and lucid account of the Registrar General's social classes and of the use of regression equations, which will be welcomed by many medical readers. Dr. Cheeseman's treatment of regression equations would probably be somewhat tedious for a mathematical reader, but to say this is to confirm that he has succeeded in describing methods unfamiliar to most medical men in a manner which makes them "intelligible to those not accustomed to think in mathematical terms".

Two chapters on infant mortality and allied subjects are also mainly statistical. The assembling and ordering of a wealth of data in these chapters is something few readers could conveniently do for themselves. Medical officers, pædiatricians and others will obtain from the fifty-odd pages a great deal of valuable information for a small expenditure of reading-time.

Anyone, however, who holds the mistaken belief that social medicine is a desiccated aggregate of figures, sums and numerical conclusions needs only to read the reasoned plea of Prof. Stevenson for a square deal for the 'girl in trouble'—a plea concluded by the observation that "the problem is one of Disabled Persons and of Rehabilitation rather than one of moral reform"; or for that matter he needs only to turn to the examination of the social and economic disorders inimical to family life in relation to the provision of day nurseries for young children. In the middle chapters of the book both these subjects are dealt with, namely, "The Health of Children in Communal Day Nurseries", and "The Unmarried Mother and her Child"; and there is also a chapter on "Problem Families". Though all these chapters are substantiated by numerical data, they convey to the reader the essential humanity of the social medical approach to some of the important problems of our day. A short note towards the end of the book on psychosomatic illness has not the clarity which marks other chapters—but, perhaps, this is scarcely surprising. The first chapter on the "Measurement and Growth in Children" makes somewhat heavy reading; it is, perhaps, one to be left for an hour of greater effort than the rest of a highly readable book requires.

Prof. Stevenson never shirks stating his views in firm terms, and this feature of the book is both refreshing and stimulating, whatever minor differences of interpretation the reader may arrive at on the facts. References given chapter by chapter are a valuable feature for students or research workers; and, all told, the book should not only be on the library shelves of medical officers of health, pædiatricians, D.P.H. and D.C.H. students and social workers, but should also be read with pleasure by all of them before being placed there.

F. GRUNDY

Traits of Divine Kingship in Africa

By the Rev. P. Hadfield. Pp. vii+134. (London: Watts and Co., Ltd., 1949.) 8*s*. 6*d*. net.

THE author introduces this volume by giving in brief the story of Osiris. He apparently takes it for granted that agriculture began in Egypt; the divine king originated in a man of genius who discovered the secret of controlling the waters of the Nile and came to be regarded as controlling the forces of Nature; royal incest and human sacrifice both originated in this Egyptian cult. In the succeeding chapters various features of many African cultures are taken and referred to the corresponding practices in ancient Egypt. The author deals in this way with the functions of royalty, with the killing of the divine king, with the king's double (the placenta, umbilical cord, *ka*, etc.), with the life after death, and with other religious beliefs and practices of this kind.

In his conclusion, summarized in Chapter 14, the author seems to recoil from the extreme Egyptocentric assumption from which he starts and to recognize the possibility of the diffusion of parts of the divine king complex from Asia, though he rejects the views of Seligman as to its association with early Hamitic invaders. In an appendix he gives a brief compendium of African ethnic groups.

For the distribution of various traits of kingship in Africa, this is a useful little book. But it takes no account of the very close parallels to African beliefs and practices which occur very much farther afield. Royal taboos closely parallel to many of those mentioned of Africa are familiar enough in South-East Asia, Indonesia and Oceania. Superstitions connected with the jawbone, in regard to which a parallel is drawn between Egypt of the Sixth Dynasty and Uganda, reappear also not only in West Africa but at any rate in the Naga Hills, in Indonesia and in New Caledonia. The complex of beliefs in divine kingship is spread far wider than the African Continent.

J. H. H.

Man the Tool-Maker

By Dr. Kenneth P. Oakley. Pp. 98+2 plates. (London: British Museum (Natural History), 1949.) 2*s*. 6*d*.

THE writer of an elementary book on prehistory for the beginner can approach the subject from one of two directions. Man is a child of Nature and appeared on this globe probably by the end of the Miocene period of Tertiary times. His early evolution coincided with the Great Ice Age, itself coterminous with the pleistocene epoch of the geologist. The early story of mankind, then, can be studied by the geologist working, as it were, upwards. Alternatively, the story can be developed by the historian working downwards from the written word to the uncharted prehistoric period. As the outlook of the prehistorian and the methods of study employed are purely geological, especially for the very early periods, the former approach would seem to be the more satisfactory.

Dr. Kenneth P. Oakley is a trained geologist and a first-rate prehistorian for the quaternary period. His little work, "Man the Tool-Maker", is altogether admirable for the purpose for which it has been compiled. Having been written by someone who really knows the subject, and not merely by a 'scissors and paste' author, as is so often the case with elementary works on the subject, the specialist will find not a few facts new to him and some lines of thought that will be of interest. Naturally, the book deals mainly with the industries, their typology and technology, but even while treated strictly scientifically, Old Stone Age man does come before us as really existing, and not merely as a fossil. Dr. Oakley has made the dry bones live.

The Songs of Insects

With Related Material on the Production, Propagation, Detection and Measurement of Sonic and Supersonic Vibrations. By Prof. George W. Pierce. Pp. vii+329. (Cambridge, Mass.: Harvard University Press; London: Oxford University Press, 1948.) 27*s*. 6*d*. net.

ALL lovers of Nature, and fortunately most of us come into that category, must welcome the application of physical methods to the investigation of living creatures. Few things are more delightful than listening to the songs of birds; one of those is making accurate records of these songs so that they can be heard by our friends and so that the song of one bird can be compared with that of another.

In this book it is not bird songs but insect songs which are being recorded, although the last chapter does deal briefly with birds and bats. The author describes the method of sound production by insects and the apparatus which he designed to record the sounds. He then gives a list of the many insects which have been investigated, including crickets, katydids and locusts. Almost all the photographs are extremely good; they include whole insects, parts of insects, recording apparatus and records. There are two omissions which are to be regretted—little is said about the hearing methods of insects, and there are no indexes either of contents or of references. It is hoped that these omissions will be made good in the next edition of this interesting book.

H. HARTRIDGE

A Text Book of Theoretical and Inorganic Chemistry

By F. A. Philbrick and Dr. E. J. Holmyard. Revised in collaboration with Dr. W. G. Palmer. Pp. vii+854. (London: J. M. Dent and Sons, Ltd., 1949.) 13*s*. 6*d*.

THIS edition contains forty-nine more pages than the revision of 1941. The authors retain the same sequence of paragraphs and the same division into chapters. The more extensive revision of Part 2 ("General and Theoretical"), undertaken solely by Dr. W. G. Palmer, increases this section by thirty-seven pages. Items of new matter added to Part 3 ("Elements and Compounds") amount to six pages.

Advances in our knowledge of photochemistry, surface action and nuclear physics have made it very difficult to obtain a balanced and comprehensive view of modern theory. Dr. Palmer has brought to his task singular powers of selection and exposition. He has re-written many paragraphs and added new ones of great significance, such as those on nuclear action, the spin of particles, the electron-pair bond, mesomerism and the inhibition of reactions. He has re-cast the chapter on valency and doubled its size; and he has been drastic in bringing the bibliographies up to date.

In giving his energy to this revision, Dr. Palmer has done students a great service; for Part 2 now affords one of the best short accounts of modern theory hitherto published and assures the position of the book in the front rank of small advanced texts.

G. F.

SCHMIDT–CASSEGRAIN TELESCOPE AT DUNDEE

By Dr. E. FINLAY-FREUNDLICH

THE impression is sometimes produced that the advance of astronomy is similar to the breakthrough of an army, when the mobile forces, breaking through the gap, rush ahead and success is measured according to the number of miles reached beyond the enemy's lines. During this advance the enemy army, with all its strong-points, is left practically intact in the rear of the advancing units. If this picture were true, it would scarcely be worth while for European astronomers to continue in observational astronomy; they would have to confine themselves to purely theoretical discussions of the sensational discoveries made by advancing units of astronomers who have the good luck to be provided with the largest telescopes.

The true picture is, in fact, very different. The great advances which astronomy has made with powerful Newtonian telescopes during the past twenty-five years, penetrating distances whence light has to travel many hundred millions of years to reach the earth, only outlines the vastness of the problems astronomy will have to solve. The real problem remains the careful, detailed investigation of all the objects discovered in the universe during this rapid advance, their distribution in space, their structure, their relationship to each other, etc. For this systematic research the powerful Newtonian telescopes are not the most suitable tools, and here abundant possibilities of research remain for all astronomers who are forced to content themselves with telescopes of modest size.

The characteristic feature of the Newtonian telescope, equipped with one parabolic mirror, is its great space-penetrating power. It is the ideal telescope for the rapid advance into unexplored depths of the universe. To this advance we owe, apart from our knowledge of the existence of millions of distant independent star systems which populate the universe, the fact also that the galaxy to which our sun belongs is a member of a 'local group' of star systems which number at present about a dozen. Nothing characterizes in more startling fashion the vastness of the progress of astronomy than the fact that this 'local group' extends to about one million light years from the sun. These star systems are still unconquered 'strong-points' left in the rear of the major advance. They reveal a great variety of structures. Among the local group are the large Andromeda system, a spiral system occupying an area of more than 3° in diameter, the large Magellan Cloud apparently quite irregular in shape, occupying an area of 12° in diameter, the great, slightly elongated, globular cluster N.G.C. 185 of 14′ diameter. The linear diameters of these systems range from 0·86 to 12·9 kiloparsec, or in the ratio of one to fifteen; this difference is accompanied by a complete, and by no means yet understood, change of structure. What is the relationship between these objects which are apparently so different? What has determined their structure, and from what initial conditions did they originate?

To study the structural features of such star systems, telescopes are needed which give on a photographic plate a good picture of the whole system; by a good picture I mean that the star images over the whole area occupied by the star system, from its centre to the boundary, must be equally well and sharply defined so that, for example, comparative photometric discussions are possible. A Newtonian telescope gives a satisfactory definition only within a few minutes of arc of the optical axis; it consequently does not satisfy this demand.

The necessity of a systematic and detailed study of the structure of the various types of star systems in the universe requires a new type of telescope. If one carefully inspects one of the beautiful pictures of star systems obtained with the 200-in. telescope—for example, that of the spiral system N.G.C. 628, published in the March issue of *Sky and Telescope*—one sees that although this system is only 8′ in diameter, star images only about one minute of arc outside it on the plate reveal images that are strongly deformed by coma. An accurate photometric investigation of the distribution of the light emitted by the various parts of the system in question would not be possible from such a picture. This deficiency of the 200-in. telescope was naturally well known from the beginning to its designers. They intend to counteract it, when extended objects are to be studied, by introducing a correcting lens specially devised for the purpose of enlarging the usable field of the telescope. However, a fully satisfactory means for correcting the optical aberrations of a reflecting system—here the parabolic mirror—by a complementary refracting system of lenses is apparently not obtainable. A better way of widening the field of a reflector had to be developed. This was discovered by B. Schmidt, who introduced thin plates, Schmidt plates, as correcting systems into astro-optics. These plates, being thin, do not introduce chromatic aberrations to a critical degree. Their purpose is not to act like a lens producing an image in its focus; placed like a window at the entrance to the telescope, in the

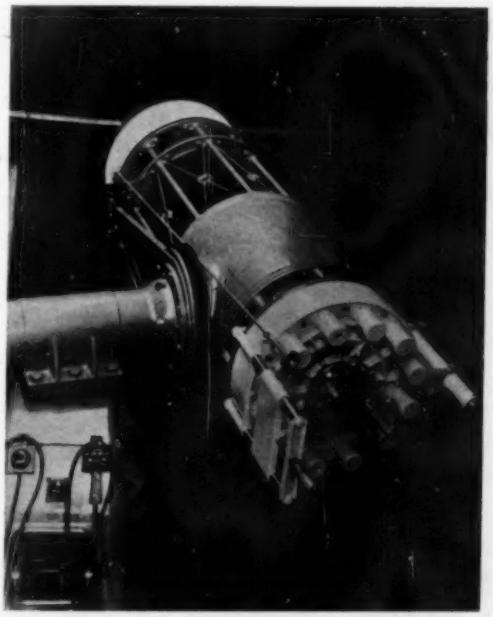

Fig. 1. The Schmidt–Cassegrain telescope mounted in the Mills Observatory, Dundee

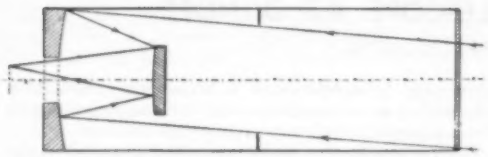

Fig. 2. Scale drawing showing proportions of the optical system of the 15–18 in. Schmidt–Cassegrain at the Mills Observatory, Dundee. The correcting plate is shown as a plane-parallel plate, the departure from flatness being only 0·0066 in.

centre of curvature of a concave spherical mirror, they distort the entering parallel light-beams arriving from very distant celestial light sources in such a manner that the reflected rays are practically free from aberrations—in particular from spherical aberration—which is the main aberration affecting spherical mirrors. To do that, one surface of the Schmidt plate is made a complex surface of the fourth order. For the practical optician this represents an extremely intricate job, because the deviation of such a surface from a plane flat does not usually amount to more than a few thousandths of an inch.

The combination of a spherical concave mirror and a Schmidt plate in its centre of curvature—now frequently called the 'classical' Schmidt system—solves from the purely optical point of view the problem of widening the field of a reflector. A classical Schmidt system may give star images of perfectly good definition over a field up to 20° in diameter. It is thus not surprising that the introduction of Schmidt correcting plates actually has revolutionized the optics of telescopes. However, seen from the point of view of the observing astronomer, the classical Schmidt system does not yet represent a completely satisfactory solution of the problem of a telescope combining the great space-penetrating power of a reflector and a wide field.

Fig. 3. Starfield in the galaxy near Perseus. Exposure 1 hr.; the field of 5° × 5° shows about 20,000 stars

The first not quite satisfying property of the classical Schmidt system is that the focal surface is not a plane but a spherical calotte. The photographic plate must be curved to fit into the focal surface along which the images are good. Secondly, the focal surface is not easily accessible; it lies inside the telescope half-way between the mirror and the Schmidt plate. Visual observations are thus not possible, and also other methods of observation, for example, the spectrographic observations of the light emitted by a celestial body, are difficult to perform.

The Schmidt–Cassegrain telescope recently completed in the optical and mechanical workshops of the University Observatory in St. Andrews attempts to remove these two inconveniences. It is the first telescope of this kind. The completed pilot model (see Fig. 1) has a free aperture of 15 in.; the focal ratio is $f/3$. The primary concave spherical mirror is 19 in. in diameter and has a radius of curvature of 49·5 in.; it has a central hole of 6½ in. diameter through which the light, reflected from the second (Cassegrain) mirror of 9 in. diameter and 47½ in. radius of curvature, falls upon the flat focal plane situated about 8 in. behind the vertex of the primary mirror (see Fig. 2). The flat field of the telescope covers 5° × 5°, which is for all practical purposes sufficient; within this area the star images are of perfect definition (see Fig. 3). Normal plane photographic plates can be used. Visual observations, as formerly performed with a refractor, are equally possible. Subsidiary instruments, such as photometers and spectrographs, can be easily connected with the telescope since the focal plane is easily accessible. Also spectroscopic work using an objective prism, which could be placed before the Schmidt plate, would be possible.

This extremely versatile and at the same time powerful type of telescope can be manufactured at relatively modest costs; it should close the gap which the Newtonian telescopes are not able to fill.

The optical theory of this Schmidt–Cassegrain telescope has been developed by Dr. E. H. Linfoot, of Cambridge, under whose directions the necessary calculations were made. The optical grinding and polishing and all mechanical work was done by R. L. Waland at the Observatory in St. Andrews. The new telescope is mounted in the Mills Observatory in Dundee.

SOVIET GENETICS: THE REAL ISSUE

By Prof. N. I. NUZHDIN
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DR. JULIAN HUXLEY chose as the cardinal point of his article under the above heading[1], and consequently also of his book[2], the problem of the "supraclass" and "supranational" character of science. "There is now a party line in genetics. . . . A great scientific nation [that is, the U.S.S.R.] has repudiated certain basic elements of scientific method, and in so doing has repudiated the universal and supranational character of science." According to the author, that "is the major issue". It is on this question that he concentrates his attention, whereas the concrete problems of biology he relegates to the second place. He writes, ". . . it is subsidiary whether or not Lysenko's claims to have made cer-

tain new discoveries are substantiated, and whether his theories are wholly or partly sound".

This statement is very symptomatic. Within the last five years many articles on problems of biology have been published outside the Soviet Union. Not only special scientific journals, but also newspapers, literary reviews, even sport and fashion journals have found it necessary to give space to articles on biology and genetics. The reasons for such a heightened interest in genetics are far from scientific. Suffice it to say, that all articles written by opponents of Michurinian science, be it men with or without scientific degrees, are stereotyped. They consist of statements against the U.S.S.R., against Soviet science and individual Soviet men of science; not one discusses scientific problems. Such articles deal with everything but the issue of the controversy between biologists. They give no idea of what Michurinian science is. This holds true also in the case of Huxley, who in the very first lines of his article declares that the questions at issue in biological science are "subsidiary".

This statement alone is sufficient to make it clear that all the trouble about biology raised by Huxley, Muller and others was called forth certainly not through solicitude for science and its development, although they try to make it seem so. Actually, it is just another link in the general campaign which the reaction is waging against the Soviet Union.

Indeed, why is it that Huxley and his adherents refuse to discuss problems of science? Why is it that when they do touch upon Michurinian science, it is either to belie it, or at best, to repudiate it with disdain?

The opponents of Michurinian science reject a scientific discussion because they have nothing to offer in its place. They cannot come with their scientific theories to the broad masses, to the core of the people, whom science is called upon to serve. Morganism (or "neo-Mendelism" as Huxley calls it), as any other reactionary trend, is against the people. Morganism not only has nothing in common with the interests of the people, but also its aims and tasks, its plans for the immediate and the distant future are against the interests of the people. That is why Huxley and his colleagues never compare the postulates of Michurinian science with those of the Morganists but prefer to keep silent on the matter. That is why both in his article and in his book Huxley profusely cites the resolutions adopted by scientific institutions in the U.S.S.R. and the utterances of Soviet men of science to the effect that Morganism is a reactionary teaching and that a fight must be waged against it; but he, for some reason, omits to say why they consider it reactionary. He offers this collection of excerpts as a documentary proof of the supposed absence of freedom of scientific development in the Soviet Union.

How much simpler and more comprehensible would it have been to the reader had Huxley written that in the Soviet Union Morganism is being routed as a reactionary trend in science, because its theoretical postulates lead to the following conclusions:

(1) That it is impossible to regulate the nature, the heredity of living organisms. Man cannot obtain directed hereditary changes as Michurinites maintain; to use Muller's expression—"life is like a game of cards".

(2) That heredity is not a property of living matter as Michurinites maintain; but is determined by a specific germ plasm. As Conklin puts it, the body is the mortal carrier of germ plasm, the mortal custodian of immortal matter—this immortal matter being called the gene; according to Muller, myriads of these genes are constantly giving out those operative, well-regulated forces which are constantly escaping from us and which build up and destroy the living world.

(3) That man is incapable of mastering these all-powerful genes. Heredity is fatal. There are good genes and there are bad genes. In man the bad genes are spread among the broad masses of working people, also among Negroes, Jews, Malayans, etc. The latter are a threat to society of to-day. According to K. Sax, poverty and ignorance are the result of bad heredity; that is why the extreme prolificacy of this group is leading to the decrease of the average abilities of the population.

(4) That, in order to avoid this, Morganism recommends two measures: (*a*) the sterilization of persons with bad heredity. Popenoe (1934) wrote that the German law on sterilization issued on January 1, 1934, was not for the improvisation of the Nazi regime; it was prepared by the development of eugenics as a science. Nachtsheim (1949) protests against throwing eugenics as a whole overboard. He claims that eugenics was not invented by national-socialism and asserts that we cannot do without it in the future. American eugenists have calculated that in the United States alone about ten million individuals with bad heredity should be sterilized. (*b*) To prohibit the natural reproduction of this group and substitute it by artificial reproduction. Seymour (1948) wrote that artificial insemination of human beings has great practical significance and received general recognition; that, if artificial insemination is applied in conformity with the principles of eugenics, the possibility of obtaining particularly good children becomes a reality. Therefore, he concludes, positive genetics for the first time becomes applicable in practice, since artificial insemination rescues mankind from degeneration and thus "true castes might be developed" (Huxley, 1945).

Had Huxley chosen to quote such conclusions based on the theory of neo-Morganism, then their number would have by far exceeded the number of excerpts taken from the resolutions and the utterances of Soviet men of science. Furthermore, every one of these conclusions makes it absolutely clear why neo-Morganism is considered a reactionary anti-popular trend in the Soviet Union. Huxley not only fails to do this, he even tries to convince the reader that "It is of no relevance to the main issue that Mendelism has sometimes been used to justify undesirable theories and actions, such as Nazi racialist theories or the exaggerated theories of inherent class superiority put forward by certain eugenists".

Among the authors that I have cited here there is only one eugenist, namely, Popenoe; all the rest are typical Morganists; but do their conclusions in any way differ from Popenoe's? It seems that like conclusions are characteristic of all Morganists. But could it be otherwise? Eugenics and racialism appeared as a reactionary product of capitalism before neo-Mendelism; but neo-Mendelism lent them a scientific semblance. The entire Morganist theory inevitably leads to eugenics and racialism.

What I have so far said is but a small part of what characterizes neo-Mendelism; but it is illustrative. Yet Huxley, after citing some utterances of Soviet men of science and excerpts from the resolutions of research institutions characterizing neo-Mendelism as

a reactionary trend, comes to the conclusions that science in the U.S.S.R. is regarded "as an activity subordinated to a particular ideology . . . the primary sanction for scientific theory is no longer consonance with the facts of Nature, but consonance with a political and social philosophy", etc.

What science, and what facts, does Huxley refer to? He shuffles the cards so as to hide the truth from the reader, namely, that only in one country in the world—in the U.S.S.R.—have men of science the right to declare openly that progressive science is progressive science and reaction is reaction. Progressive science must be respected, developed, supported; reaction must be eliminated even if it is dressed in scientific clothes. Even Huxley agrees with this last statement. He declares that the State "can legitimately do everything in its power to check superstition, to combat unscientific or anti-scientific attitudes of mind . . ." (we may add, 'and reactionary ones, too').

Huxley considers that the men of science themselves should decide what is reactionary and what progressive, what is scientific and what anti-scientific. Of course, men of science play an important part in this respect. So it is in the U.S.S.R. The discussion on the problems of biology continued for fifteen years. But it is not only for the men of science to decide this question. Have not the people whom science serves and who support science the right to voice their opinion?

Now we can turn to the "major issue", as Huxley calls it. At great length he continues to "substantiate" his views on this "major issue" in an attempt to prove that in the U.S.S.R. science no longer bears a "supranational" character, that it is a partisan, class science subordinated to a definite political and philosophical ideology, etc. All means are used to prove this point—quotations, rumours, emigrants, traitors. Huxley collected everything to make the arguments about the partisan and class nature of science in the U.S.S.R. more convincing. And all this work was redundant, for Huxley was forcing an open door.

From the moment that Marxism made its appearance, and therefore long before the establishment of Soviet power, it taught that all political, philosophical, scientific, ethical, etc., theories always reflected and continue to reflect class interests. We firmly adhere to this Marxist principle because it is correct. That is why Huxley's statement that "a great scientific nation has repudiated the universal and supranational character of science" is not true. The "great scientific nation" repudiated nothing because Soviet science never accepted these reactionary claims; it always combated them.

We have openly declared and continue to declare that science, and therefore Soviet science, is a partisan science, a class science. We have no reason to hide it, since our science by serving the most progressive class of contemporary society—the proletariat—is a people's science. In consequence of this, it is free from the reactionary fetters that capitalism forces upon science. That is why it is the most correct science since it has no other interests than to reveal the objective laws governing the development of Nature and society.

Capitalist science which serves the reactionary class of contemporary society cannot be objective. Capitalism demands that science distorts the truth and creates reactionary theories serving capitalism as ideological tools.

The *bourgeoisie* and its ideologists, whether they be biologists or not, were always afraid of openly avowing the partisan nature of science. They tried to mask its partisan nature with "objectivism", "supraclass" and "supranational", "universal" qualities and similar absurdities in order to hide the true nature of its partisanship. As far back as 1905 V. I. Lenin wrote the following lashing words about *bourgeois* non-partisanship: "Non-partisanship in bourgeois society is but the hypocritic, veiled, passive expression of belonging to the party of the well-fed, to the party of the ruling class, to the party of exploiters. Non-partisanship is a bourgeois idea. Partisanship is a socialist idea."

All the verbal nonsense about the "supraclass" and "supranational" nature of science serves Huxley's absolutely definite class aims. Actually, he himself does not believe in the "supraclass" and "supranational" nature of science, although he attempts to depict it as some sort of a Pickwick Club broadened to a world-wide scope by calling it an international activity of free workers. After strong statements about the partisanship of Soviet science, Huxley asks: "How should men of science act in the face of the increasing concern of the State with science and the consequent increasing pressure of the State on science? Can they accept the existence of an official scientific policy? . . . Can they accept official direction as to what subjects shall be investigated?"

As things stand, the posed questions do not relate to Soviet science or to the Soviet State; but to capitalist science. The "supranational" Huxley himself has felt the constantly increasing pressure of the capitalist State machine on science. He is forced to serve a definite purpose, his masters dictate their will to him. To oblige them he published his slanderous article against the Soviet Union.

But despite his truly piteous state, Huxley continues to play the game of "freedom". He asks: Can men of science accept it?, and answers: "I think that they can". And that is not all. He further calls upon all men of science to agree that ". . . it is legitimate", and even "desirable, for a Government to embark on such a policy".

The question naturally arises what was the purpose of Huxley's philosophy of the "supraclass" nature and the "autonomy" of science?

The answer to the question is simple: Huxley himself gives it. The position of science in capitalist countries is such that Huxley is forced to beg for a little independence, for a bit of fresh air. The governments of capitalist countries throw a few crumbs to science, and the interest they demand for these crumbs is much too great. Huxley is confronted with the horrors of the committee of investigation.

In order to earn the reputation of trustworthiness in advance, he piles calumnies against Soviet science and the Soviet Union. The "supraclass" man of science openly shows his class face. He looks for means of forcing the Soviet State to change its scientific policy and suggests a sort of scientific entente. ". . . Clearly nothing can be done here save by way of precept—through an inter-governmentally agreed charter."

Any proposition in Huxley's article, whether it refers to Michurinian science or Morganism or involves any other general questions, is presented in a distorted light. For example, speaking about the fact that Morganists are called Weismannists in the U.S.S.R., Huxley writes that ". . . in the West

Weismann's particular views are now mainly of historical interest only". That is not true! Morgan wrote that neo-Mendelism is to a considerable degree obliged to Weismann for its ideas of the isolation and continuity of the hereditary substance[3]. But that is exactly one of the cardinal postulates of Morganism against which Michurinian science is waging a struggle.

When he speaks of the veracity of neo-Mendelism, Huxley declares:

"In this, large bodies of phenomena are elucidated on the basis of the following elementary facts: (*a*) the existence of discrete material units of heredity lodged in a definite order in the chromosomes. . . ." That is not true. Morganists have become muddled on this point; they now themselves renounce this statement of Huxley. Goldschmidt writes that within the last decade many geneticists have come to understand that the classical theory of the corpuscular gene, that is, the actual existence of separate bodies arranged as beads along a chromosome, does not agree with facts[4]. Stern, too, repudiates the classical picture of the genotype as "a number of beads [genes] strung up in a certain sequence on a string [the chromosome][5]."

Muller and Demerec are even forced to speak of the gene as of a structure without bounds. This does not mean that neo-Mendelism is taking the right course. It is moving toward augmented metaphysics and even mysticism, as can be judged from the above-mentioned article of Stern, where he advocates quantum genetics with its "modulated" and "unmodulated" sections of the chromosome. But that is not what interests us here. We are concerned with Huxley's misrepresentations. They send their shoots to the surface even when he speaks of the science which he personally propagates. But if that is how Huxley treats the Morganists, what then can be expected of him when he writes about Michurinian science and Michurinites?

Let us begin with Huxley's opinion of I. V. Michurin as a man of science and his evaluation of Michurin's significance in science. . . . "Michurin is being deliberately glorified as a great Russian pioneer in agricultural and biological science (whereas in point of fact he was essentially an empiricist who scored some important practical successes, but whose theoretical speculations have become scientifically negligible in the light of later research). . . ." Huxley's talent is not up to the mark. Darlington has one over him. The latter simply declared that it would be easier to assume that Michurin got his best plants from Canada and the U.S.A.[6].

As a matter of fact, Michurin created more varieties than all the Morganists put together in the whole history of their science. Michurin created more than three hundred varieties of fruit and small fruit cultures unexcelled in quality to this very day.

But Huxley ignores this. That is why he declares that "the theoretical speculations" of Michurin "have become scientifically negligible in the light of later research". I would like to ask what theoretical speculations of Michurin have proved to be unsound, and in the light of whose later researches? But Huxley would not be able to give coherent answers because he knows Michurin only by hearsay or from the words of his adherents.

The position of all other Michurinites, Huxley simply qualifies as the "unscientific attitude of all Michurinites". Why unscientific? What grounds has he for such an assertion?

Let us examine some of the "proof" of the "unscientific attitude" of the Michurinites. According to Huxley one proof is "the repudiation of the theory of probability as applied in neo-Mendelian genetics". Huxley is careful. Darlington declared simply that Lysenko repudiates statistical verification, and Nachtsheim wrote that Lysenko considers the slide rule to be mystic.

Yet scores of works by Michurinites may be cited where the methods of variation statistics are applied. For example, those in *Agrobiologia*, the *Trudy Instituta Genetiki* (*Transactions of the Institute of Genetics*), or Glushchenko's monograph "Vegetativnaya gibridizatsia rasteni" ("Vegetative Hybridization of Plants") should carry conviction of this. It is not the theory of probability that Michurinian science repudiates, but the way it is applied by the Morganists. We are against substituting biology by statistics; we are against the false utilization of statistics in biology. One of the leading Morganists who is as great a patron of statistics as Huxley, namely, Darlington (and with him others), declared that a woman's giving birth to twelve sons one after another was formerly explained by the will of God or by the passion of the man, etc., and only the discovery and elaboration of probabilities demonstrated that the birth of twelve sons is neither mystic *nor physiology*, but *pure statistics*! Michurinian science is against just such kinds of "statistical" distortions which degrade science.

A second proof of the Michurinites being "unscientific", according to Huxley, is that they make use of impure material and never set up controls. The statement refers to the cases of directed conversion of winter forms into spring forms and to the cases of vegetative hybridization. This is just as wrong as the first statement. Nevertheless, it serves Huxley to substantiate his conclusion that ". . . the hereditary conversion of winter into spring cereals by partial vernalization . . . is quite certainly not proved and almost certainly unfounded".

Huxley has only one "proof". He refers to the opinions of others—Darlington, Ashby, Waddington. Now with Huxley's article and book the number of such "proofs" has increased. Yet had Huxley glanced into the works of Michurinites relating to the questions at issue, he would have had to write that his references were unsound.

Speaking of the directed conversion of wheat, Huxley writes: "I would say with assurance that selection was *never* ruled out. . . . Without genetic purity, selection will automatically operate. . . . Furthermore, he [Lysenko] never seems to have employed adequate controls." Were Huxley to open, let us say, the sixteenth volume of the *Transactions of the Institute of Genetics* (1948), he would clearly see that what he writes is incorrect. In a paper published in that volume, V. N. Stoletov shows on the basis of experimental data the groundlessness of such statements which have been made, even prior to Huxley. He would see that the material used in the work was pure and that selection had nothing to do with it, because the plants that eared during the first year after the spring sowing of winter wheat, the so-called "upstarts", in the future continued to remain winter forms, despite their being the most suitable material for selection. Were he to look through another paper printed in the volume, the work of A. T. Trukhinova, he would no longer claim that Michurinites fail to set up a control. Without even reading the article, a mere glance at the legend

to Fig. 1 would make it clear that his statement is wrong. The legend reads as follows: "Fig. 1 to the left. Uneared Milturum 0321 plants that overwintered for three consecutive years at the Chelyabinsk GSS. To the right—control plants of Milturum 0321." By sowing spring forms in late autumn in Siberia and southern Urals, Trukhinova, who employed controls, obtained winter forms that were more frost-resistant than the world standard Lutescens-0329. Were Huxley to look through the tables on pages 146, 147, 148, 153, 154, 155 of the same volume he would see that Michurinists do apply variation statistics.

On the same grounds Huxley disputes the possibility of vegetative hybridization, but his so-called proofs are no more convincing. All potency of his proofs centres in the assertion that, "As Ashby (1948) has pointed out, the mainstay of his (Lysenko's) evidence is the original experiments of Avakyan and Yastreb (published in 1941), which were discredited in 1944". In 1948 I. E. Glushchenko published his monograph on the "Vegetative Hybridization of Plants", where one may find, in the first place, the results of the author's own investigations carried on for a period of many years, and, secondly, references to the works of scores of other investigators. Vegetative hybridization is now being applied by plant breeders in the breeding of new varieties, and Huxley still refers to the work of 1941 as to the only one.

[1] *Nature*, **163**, 935, 974 (1949).
[2] "Soviet Genetics and World Science: Lysenko and the Meaning of Heredity." By Dr. Julian Huxley. Pp. x+245. (London: Chatto and Windus, 1949.) 8*s.* 6*d.* net.
[3] "Theory of the Gene" (Russian ed., p. 30).
[4] Goldschmidt, R. B., *Experientia*, **5**, 216 (1946).
[5] Stern, C. G., *Yale J. Biol. and Med.*, **5**, No. 6, 19 (1947).
[6] *Discovery*, **8** (1947).

J. L. GAY-LUSSAC (1778–1850)

By Prof. J. R. PARTINGTON, M.B.E.

JOSEPH LOUIS GAY-LUSSAC, the centenary of whose death occurs on May 9 this year, was born at St. Léonard in the department of Haute-Vienne. His early education was interrupted by the French Revolution; but in 1797 he entered the famous École Polytechnique in Paris, where Berthollet was then professor of chemistry. He attracted the notice of Berthollet and became his demonstrator, and also assisted Fourcroy, thus coming in touch with the two leading chemists in France at the time. Berthollet set him to work in his private laboratory at Arcueil, where he also met Laplace. His first work was in physics, and as a master in both sciences he was well fitted in 1809 to become professor of chemistry at the École Polytechnique and professor of physics at the Sorbonne. In 1818 he became superintendent of the Government powder factory, and in 1829 chief assayer at the mint, in which capacity he developed volumetric analysis as applied to silver titrations. In 1832 he became professor of chemistry at the Jardin des Plantes, relinquishing his post at the Sorbonne. When Berthollet died in 1822 he left his sword, part of his costume as a peer of France, to Gay-Lussac, who achieved this honour in 1829. In later life he spent more time on technical and consultative work, being under the necessity of providing for a family; but in this field his activity was also largely experimental.

Equally proficient both in chemistry and physics, Gay-Lussac made his mark in both sciences, and by his studies on the borderline between them he became one of the founders of modern physical chemistry. In the early years of the nineteenth century he was the outstanding chemist in France, equalled only by his contemporary and rival, Davy, in England. The two were often working on the same subject, and some friction was aroused by this. In 1808 Gay-Lussac and Thenard intruded into the field of study of the alkali metals, and a few years later Davy repaid this by embarking on researches on chlorine and on iodine. In both cases the rival work led to important discoveries, and science benefited by it.

In 1801 Gay-Lussac published his experiments on the expansion of gases by heat, establishing that all gases have practically the same coefficient of expansion. Dalton simultaneously arrived at the same conclusion. In 1804 Gay-Lussac was associated with Humboldt, with whom he travelled, and in 1805 they published a joint paper on the analysis of atmospheric air by explosion with hydrogen, a method invented by Volta. In 1804 he had made a balloon ascent alone to the record height of 7,000 metres, and the sample of air he brought back had the same composition as that collected at ground-level. The experiments showed that hydrogen and oxygen combine almost exactly in the ratio of two to one by volume, and Gay-Lussac in further experiments found that equally simple combining-volume ratios held also for other gases, his famous law of volumes being read to the Society of Arcueil on December 31, 1808, and published in 1809. The obvious relation to Dalton's atomic theory was not stressed by Gay-Lussac, no doubt on account of his regard for Berthollet, who had proposed a law of indefinite combining proportions in 1803, along with the law of mass action. Dalton never accepted the law, the explanation of which by Avogadro in 1811 passed almost without notice at the time.

From about 1808, Gay-Lussac worked with Thenard at the École Polytechnique, and this was the most active period of his scientific work. They made experiments with the alkali metals, having a large voltaic battery and other research facilities provided by a grant from Napoleon, who was keenly interested in such work and anxious to match in France the brilliant work of Davy, whom he much admired. They also worked on the nature of chlorine, but were unwilling to relinquish Lavoisier's view that it was an oxide, and also developed a method of organic combustion analysis. This work was published in 1811 as "Recherches Physico-Chimiques" in two volumes, still quoted as an authority in current books. Gay-Lussac's investigations of iodine, published in 1814, were made simultaneously with Davy's and with the same material, discovered by Courtois in 1811. This work shows his capacity for experiment at its best. His researches on cyanogen in 1815 showed that this was an organic radical, and also provided another example of a substance combining with hydrogen to form an acid. In 1816 he distinguished between the two oxides of nitrogen, N_2O_3 and N_2O_4, and in 1819 in collaboration with Welter he discovered dithionic acid.

As a pupil of the physical chemist Berthollet, Gay-Lussac was able to appreciate the parts of Berthollet's views which were of permanent value. He emphasized the similarity between solution and evaporation, and realized that in solutions of mixed salts the radicals were interchanged in what he calls a "pêle-mêle". It

was only long after his time that these two ideas came into prominence in the new theory of solutions of van't Hoff and Arrhenius.

Gay-Lussac also made important contributions to chemical technology. In 1821 he showed that wood impregnated with borax is non-inflammable; in 1827 he invented the Gay-Lussac tower, which is still an essential part of the sulphuric acid chamber plant in serving to retain the oxides of nitrogen circulating through the chambers; and in 1829 he showed that an oxalate is formed by fusing sawdust with caustic alkali. He devised a burette and worked out volumetric methods for the determination of acids and alkalis, of chlorine in bleaching powder, and of silver by chloride titration, all of which are in current use.

Unlike his collaborator Thenard, Gay-Lussac did not write a formal text-book on chemistry; but his lecture notes, taken down in shorthand, were published in two volumes in 1828, those on physics being also published in the same year.

Sir Robert Christison, who attended his lectures in 1820, says that Gay-Lussac had a slender and handsome figure, a comely face and a winning expression. His voice was gentle and persuasive but firm and clear, his lectures being "a superlative specimen of continuous unassailable experimental reasoning". He married in 1808, meeting his future wife, then aged seventeen, in a linen-draper's office where he saw her reading a book. On asking what it was, she told him it was on chemistry. He had her educated in English and Italian, and during the forty years of their married life their interests were so close that it is said that even their handwriting was almost identical, it being impossible to distinguish a copy of a manuscript made by Mme. Gay-Lussac from the original of her husband.

The first half of the nineteenth century saw France pre-eminent in nearly all branches of science. Great mathematicians, physicists, chemists and physiologists taught and worked in Paris, an acknowledged centre of intellectual brilliance which attracted workers from all over the world. Among its greatest names, that of Joseph Louis Gay-Lussac has a proud place, and his laurels will ever be green.

OBITUARIES

Prof. N. Herman Nilsson-Ehle

NEWS has been received of the death, on December 29, 1949, of the eminent Swedish plant breeder and geneticist, Herman Nilsson-Ehle, at the age of seventy-seven, after a life devoted to research for the betterment of agriculture and the advancement of fundamental science. Nilsson-Ehle was not only held in the very highest esteem in his own country, but he also had a great international reputation. With his death Sweden loses one of the figures that dominated the field of crop-breeding and genetic research for many years; even in retirement he continued to stimulate work and thought in many aspects of plant improvement. Nilsson-Ehle's name will, however, always be associated with the splendid work and organisation of the Swedish Seed Association at Svalöf, the growth of which he directed during a period of the greatest importance to its history and of the utmost significance for the development of plant-breeding and genetics outside his own country.

Nilsson-Ehle received his academic training at the University of Lund, and after graduating, when he was still known as Herman Nilsson, he took part in an expedition to Siberia. On returning to Sweden he was appointed to Svalöf, as plant breeder specializing in wheat and oats under Hjalmar Nilsson—a position he held until 1915. During this time he made very important contributions to the theory and techniques of plant breeding, and was responsible for introducing breeding methods which not only became routine at Svalöf but also were accepted in other countries. As a result of the application of these techniques, several important wheat varieties were bred at Svalöf and were introduced with success into Swedish agriculture.

It is difficult to separate Nilsson-Ehle's contributions to practical breeding from his researches in genetics. His study of the inheritance of important economic characters such as yield, hardiness, earliness, disease resistance and grain colour were instrumental in developing the theories of quantitative character inheritance and the action of polymeric factors which are of such significance to the breeder. With the acceptance of the concept of transgressive segregation, and the more intelligible interpretation of the effect of environment on complex multiple-factor inheritance, breeding was put on surer ground, and hybridization as a means of crop improvement received considerable stimulation. During this period Nilsson-Ehle conducted intensive studies on large-scale hybridizations of wheat and oats, and with the development of genetical concepts came new ideas on breeding techniques.

In 1910 he proposed and introduced a new and simpler method of handling hybrid progenies, by which pedigree selection and yield tests were carried on side by side during segregating generations. This method is still used at Svalöf, as also is the bulk or population method devised by Nilsson-Ehle and introduced a few years earlier. By this latter method, selection is deferred to the later generations when a high degree of homozygosity has been reached, and although not generally approved or used, it has certain advantages, particularly in the exploitation of natural selection when such is desired. It is true to say that no revolutionary changes have been made in the technique of handling hybrid populations in self-pollinating crops since these innovations, and most breeders use these techniques or modifications of them.

In 1915 Nilsson-Ehle was appointed to the chair of botany at the University of Lund, and two years later this was changed to a personal professorship of genetics with an institute at Åkarp. During this period he continued his investigations on the genetics of cereals until in 1925 he was recalled to Svalöf to succeed Hjalmar Nilsson as director. Nilsson-Ehle directed the activities of Svalöf for fourteen years, during a time of considerable expansion of the scientific and practical breeding activities of that important centre of research which has become a model of its kind. Not only has Svalöf contributed improved varieties of crop plants—particularly oats and wheat—which have played an important part in raising the productivity of Swedish agriculture, but also some of these varieties have been introduced with profit to other countries, including Britain.

Among other developments at Svalöf due to Nilsson-Ehle's inspiration was the founding of a cytogenetic department in 1931 "for research on chromosomes and the breeding methods based thereon". The realization of the significance of polyploidy in the phylogeny and evolution of culti-

vated plants, and the association of certain important biological characters with polyploidy, led to speculation about the possible application of artificially inducing polyploids—both autopolyploids and allopolyploids—to plant breeding. The cytogenetic department at Svalöf developed these ideas, and with the discovery of the colchicine technique considerable impetus was given to both the fundamental scientific aspects and the breeding approach. In addition, work was developed on the artificial induction of mutations, a problem which had attracted Nilsson-Ehle since his studies of naturally occurring mutant forms in wheat, barley and rye, and on which he had published many papers.

After his retirement, Nilsson-Ehle continued to be actively interested in plant-breeding. It was due to his energetic interest that institutes for the improvement of forest trees and fruit trees were founded, while he himself devoted time to the breeding of the giant aspen. His great energies and capacity for work have left their mark on scientific research, on agriculture through crop improvement, and on the institutes which he guided. In the academic sphere his long association with the lecture room and laboratory inspired and stimulated a great number of students and research workers who came under his influence, and it is gratifying to know that his name was honoured by the creation of a fund in 1943, the revenue from which will be used for stimulating genetics research. G. D. H. Bell

Prof. G. H. Livens

With the unexpected death of Prof. G. H. Livens on March 26, the University College of South Wales and Monmouthshire, Cardiff, has lost not only a distinguished mathematician, but also one who has rendered valuable service to the College for the long period of twenty-seven years.

Born in 1886, he received his early education at the Latymer Upper School, Hammersmith, before proceeding to Jesus College, Cambridge. At Cambridge he had a distinguished career, being bracketed fourth wrangler in Part I of the Mathematical Tripos of 1909, the last year in which the lists were published in order of merit, and being placed in Class I, Division 2, in Part II in 1910. In 1911 he was awarded the first Smith's Prize, and in the same year he was elected a Fellow of Jesus College.

Meanwhile he had been appointed lecturer in geometry at the University of Sheffield, where he stayed until 1919, when he became senior mathematical lecturer at the University of Manchester. He was elected to the chair of mathematics at Cardiff in 1922.

It was during his period in Sheffield that Prof. Livens started to publish numerous papers on electrical theory, and his "Theory of Electricity" was first published in 1918. Later he published papers in many branches of applied mathematics, but chiefly in electrical theory, magnetism and thermodynamics.

In educational affairs generally, Prof. Livens took a very active part. Besides his departmental duties, he gave loyal service in the faculties and the Senate, and in the many committees on which he served he was always much esteemed, not only for his personal integrity and sincerity, but also for the valuable contributions he made to the business under discussion. His decisions were made without prejudice, and with no thought of personal interest.

Prof. Livens's interests extended beyond the University, however, in many directions—to the mathematical courses in the Welsh training colleges, to the Central Welsh Board as an examiner and adviser for many years, to the local branch of the Mathematical Association—and it may well be said that he did great service in guiding the development of the study of mathematics in Wales. His value as an examiner is also shown by the fact that he was an external examiner for the University of London from 1929 until his death.

Prof. Livens will be remembered with affection by many generations of students who always found him willing to be helpful; his staff will remember with gratitude his continual readiness to discuss all aspects of their departmental and research work, and also the value of his advice and guidance.

R. M. Morris

Dr. F. W. Foxworthy

Dr. Fred William Foxworthy, forest research officer during 1918–32 in the Federated Malay States, died suddenly in Berkeley, California, on February 4. He was the younger son of a Methodist minister, and was born on July 7, 1877, at Goodland in the State of Indiana. His education to 1899, when he graduated in science at the De Pauw University, was within his native State; then he proceeded to Cornell, where he was given a teaching post and took the degree of Ph.D. in botany (1902). Many knew him in those college days as a boxer, a football player and a redoubtable baseball player. In 1906 he was appointed a botanist in the Bureau of Science, Manila, and gradually became a forester: a dominating interest in the ecology of the forests led to it.

The trees having determined his bent, he was made wood technologist in the Bureau and associate professor of dendrology in the University of Manila. It was natural that he should turn from the Philippine forests to compare them with those of other parts of Malaysia; and, with the conservator of forests, British North Borneo, he published in 1917 an account of the mangrove and nipa swamps of northern Borneo through which runs a comparison of these formations in the two places. In 1918 he accepted the appointment of forest research officer under the Government of the Federated Malay States. This new post brought to him unrivalled opportunities for work; and he used them to great advantage: splendid undamaged forests were coming under control, and the large active staff controlling them was ripe to aid in collecting specimens and making observations; his part was to sift, to direct and to attach the technology to the botany. Building up a small museum of forest products and a forest herbarium, he had by 1927 materials for his "Commercial Timbers of the Malay Peninsula" (*Mal. For. Rec.*, No. 5) and, on a narrower front, by 1932, materials for his "Dipterocarpaceæ of the Malay Peninsula" (*Mal. For. Rec.*, No. 10): at various dates there were less exacting publications. The second of the two named greatly advanced our precise knowledge of the chief sources of Malayan timber.

Retired at his age limit, Dr. Foxworthy went back to the United States and, marrying, settled at Berkeley, California, living actively, teaching, writing, joining in the scientific life of the neighbourhood of San Francisco, and during the Second World War called to war work. In 1936 he was president of the California Botanical Society. Later he wrote for the

Chronica Botanica Publishing Company a book which will shortly appear under the title "Forests and Forestry of Tropical Asia" embodying his wide experience. In the course of working on it, he built up a great index of references to forest products—references to ten thousand species of plants, he told the writer; and this index, he added, if fated to go unprinted, is to be turned over to one of the forestry centres. I. H. BURKILL

WE regret to announce the following deaths:

Prof. A. B. Appleton, emeritus professor of anatomy at St. Thomas's Hospital, University of London, on April 22, aged sixty-one.

Mr. F. F. R. Channer, C.I.E., O.B.E., sometime chief conservator of forests, India, aged seventy-four.

Mr. J. B. Scrivenor, I.S.O., formerly director of the Geological Survey, Federated Malay States, on April 21, aged seventy-three.

NEWS and VIEWS

"Soviet Genetics: the Real Issue"

THE publication of Dr. Julian Huxley's two articles under this title in *Nature* of June 18 and 25, 1949, provoked widespread interest. Such a topic could clearly not be allowed to become the subject of debate in the correspondence columns of this journal; although feeling might run high, there can be relatively few who have first-hand knowledge to bring to the discussion. Dr. T. Marchlewski, writing from Cracow, claims such direct knowledge, and a short communication by him, followed by Dr Huxley's reply, appears on p. 731 of this issue. A very full statement of the Soviet point of view was also received from Prof. N. I. Nuzhdin, of the Institute of Genetics, Academy of Sciences of the U.S.S.R. Much of this was in terms which, although perhaps regarded in the U.S.S.R. as the normal way of conducting a controversy, would appear merely offensive when translated into English. The article was accordingly abridged by the Editors, and a proof of the shortened version submitted to Prof. Nuzhdin in Moscow. The following reply, dated April 13, has now been received: "Regret delay in answering your letter March twenty-first in view necessity communicate with Professor Nuzhdin in China. Received his reply to-day. Professor Nuzhdin consents publication article as given in galley proof but extremely regrets fact that editors deem it impossible publish article in original form written by him. On authorisation of Professor Nuzhdin, Lavrova secretary". The abridged article is therefore published in this issue of *Nature* (see p. 704).

Psychology at University College, London: Sir Cyril L. Burt

SIR CYRIL BURT was elected to the chair of psychology in University College, London, in 1931. He now retires after a period of most distinguished service, one of the acknowledged leaders in his subject, not only in Great Britain, but also throughout the world. Already, before his selection as successor to the late Prof. C. E. Spearman, Sir Cyril had developed all three of his main official psychological interests: in methods and devices of intelligence and other forms of tests, in statistics applied to psychological tests and experimental results, and in the fundamental problems of juvenile delinquency. During the past eighteen years he has consolidated his position and greatly broadened his contributions in all these fields, but more particularly in the field of psychological statistics. He himself and the very large number of students who have been guided and inspired by him have made by far the most striking contributions of the day to the theory and practice of factor analysis; and not only in his book "The Factors of the Mind", but also in a large number of more recently published articles, he has provided a broad psychological foundation for this method of approach which will be a permanent memorial to his years of leadership at University College. Apart from his more official labours, Sir Cyril has endeared himself to a very large number of students and colleagues by his unfailing sympathy, humour and insight, and by his very wide interests extending far beyond academic boundaries into a broad range of contemporary problems.

Dr. R. W. Russell

DR. ROGER WOLCOTT RUSSELL, who will succeed Sir Cyril Burt, is a native of Worcester, Massachusetts, and he graduated at Clark University, Worcester, so long associated in the minds of psychologists in Great Britain with Stanley Hall, its first president. There, Dr. Russell took his master's degree in psychology in 1936, and later his doctorate at the University of Virginia. During the Second World War he was a research psychologist in the United States Army Air Force School of Aviation Medicine and was engaged on personnel selection and other inquiries, which brought him on two occasions to work with units serving in Great Britain. After the War he became associate professor in the University of Pittsburgh. In June of last year he was appointed to a Fulbright advanced research scholarship by the United States Department of State, and has since then been working as director of the new Animal Research Laboratory established by the University of London at the Institute of Psychiatry. Dr. Russell's earliest investigations were mainly in the field of child psychology, on which he has published numerous papers. He has also published some of his investigations carried out during the War, notably a study of the effects of anoxia on mental dexterity, and results obtained with psychological procedures for the selection of officers for the Air Force. During the past two years he has been carrying out investigations in animal psychology, largely with the view of throwing light on the effects of electro-convulsive shock as administered to human beings.

Zoology at Queen Mary College, London: Prof. A. J. Grove

PROF. ALFRED JOHN GROVE, who retires from the chair of zoology at Queen Mary College (University of London) at the end of the present session, is a graduate of the University of Birmingham, where he was a contemporary of his friend and collaborator in the well-known series of dissection manuals, Dr. R. H. Whitehouse. Prof. Grove left Birmingham with an 1851 Exhibition research scholarship for Emmanuel College, Cambridge, where he took his B.A. by research. This was in 1919; but before that he had seen service as an entomologist in the Indian

Agricultural Service during 1911–15 and as entomologist to the British and Allied troops in Mesopotamia during 1916–17. From this latter appointment he was recalled to investigate the prevalence and control of malaria-carrying mosquitoes in Great Britain, and on completion of this work he joined the staff of the University of Sheffield, where he remained until 1929, by which time he had begun his well-known work on the breeding habits of earthworms. In 1929 he was appointed lecturer in zoology at Queen Mary College, London, became University reader and head of the Department in 1937 and University professor in 1947. For many years he was dean for medical students, but for the past two years he has acted as dean of the Science Faculty. Co-author of many text-books on zoological subjects and an authority on the breeding habits of Oligochætes, he will perhaps be best remembered by his colleagues in London for his work in building up the Zoology Department at Queen Mary College. The staff and students of the College will greatly miss his kindly interest in their many activities.

Dr. J. E. Smith

THE election of Dr. J. E. Smith as the successor to Prof. A. J. Grove is something more than a well-earned personal distinction, for it strengthens the bond of friendship established during the Second World War between the Departments of Zoology at Queen Mary College and Cambridge. Dr. Smith will need no introduction to his new staff, for they know, at first hand, both his ability as a teacher and his outstanding loyalty as a colleague. Dr. Smith has had an unusually wide range of teaching and research experience; trained at King's College, London, he became a student probationer at Plymouth in 1930, and, after serving at Manchester and Sheffield, he was appointed lecturer and curator of invertebrates at Cambridge in 1938. As a teacher he has an almost unique ability of stimulating all grades of students and of putting life into what might otherwise be a somewhat unresponsive class. In the field of research he has opened up new and fruitful ground in the neurology of the echinoderms; it is to be hoped that he will be able to devote more and not less time to this most interesting work. Dr. Smith has, in addition to a heavy load of teaching and research, invariably been willing to carry his full share of administrative responsibilities. He was for several years both administrative officer of the Zoological Department at Cambridge and secretary of the Faculty Board of Biology. In both these capacities he will be remembered not only for his ability but also for his unfailing tact and good humour. Dr. Smith's new appointment constitutes a very serious loss to Cambridge zoology; but he carries with him not only the good wishes of his immediate colleagues but also the universal affection of every member of his old Department.

Large-Screen Television

THE possibility of presenting television pictures of an adequate brightness on the large-size screen used in cinemas has been under development in Great Britain for some years. On April 29, the opportunity was taken by Messrs. Cinema-Television, Ltd., to demonstrate the state of this development by showing the B.B.C. Cup Final television programme to a selected audience of about a thousand persons, including television experts from some fifteen countries. The normal programme radiated from the Alexandra Palace transmitter was received in the neighbourhood of the Odeon Theatre, Penge, where the demonstration was given. The received signals were conveyed to special equipment placed in the auditorium of the theatre at a distance of some 12 metres from the screen. A very bright image of the television picture, about 16 cm. × 13 cm. in size, was formed on a special cathode-ray tube operating from a high-tension supply of 50 kilovolts. The optical projection system comprised a spherical mirror and plastic correcting plate by means of which the picture was thrown on to the theatre screen of approximate dimensions 6 m. × 4·6 m. A special metallized fabric directional viewing screen is used; this is perforated for sound and can thus be hung in front of the theatre speaker, by which the sound transmission was conveyed to the theatre audience. The projector demonstrated was designed to give a picture brightness comparable with that of normal film projection, with a high-light brightness of about 7 foot-lamberts, while the definition is naturally based on the 405-line standard of transmission of the B.B.C.

In spite of the fact that, owing to weather conditions, the daylight at Wembley was on the dull side, the demonstration was very satisfactory. Viewed from the theatre balcony at a distance from the screen of more than 30 metres, the brightness and quality of the picture were very good and enabled this annual climax in English association football to be followed with ease and great interest. No technical difficulties were experienced during the demonstration, and the equipment gave very satisfactory operation, which was quite uninterrupted except for the usual 'half-time' interval.

Inter-African Information Bureau on the Conservation and Utilization of the Soil

AT the African Regional Scientific Conference, held in Johannesburg during October 17–28 (see *Nature*, November 26, 1949, p. 901), a number of resolutions were passed for the setting up by Belgium, France, Portugal, the Union of South Africa and Great Britain of bureaux and permanent scientific committees in accordance with the policy of technical co-operation in Africa south of the Sahara. As a result of one such recommendation, the Inter-African Information Bureau on the Conservation and Utilization of the Soil has been set up in Paris. The Bureau gives practical effect to the wish of these countries to see the best use made of the land resources of Africa and to promote the economic and social development of the indigenous population. It will facilitate contact and establish permanent liaison between regional African committees on soil conservation, the Inter-African Pedological Service, and all experts and national and international bodies concerned with these problems. The address of the Bureau is Muséum National d'Histoire Naturelle, 47 Rue Cuvier, Paris.

The British Bryological Society

THE annual general meeting and excursion of the British Bryological Society was held at Totnes, Devon, during April 12–19, under the presidency of Prof. P. W. Richards. About forty members were present, including one foreign member, Prof. R. van der Wijk (Groningen, Holland). Visits were made to Holne Bridge, Wistman's Wood, Bolt Head and other places of interest. It was decided to open the Society to junior members under the age of twenty-one (or, if students, twenty-five), who would have the privileges of other members, including that of receiving the Society's *Transactions*, but not the right to vote or

hold office. A new check list of British mosses has been prepared by E. C. Wallace and P. W. Richards and will be published by the Society this year. It is hoped to hold meetings in 1951 in south-west Ireland and at Oxford.

Control of the Horsetail, *Equisetim arvense*

METHOXONE (2-methyl, 4-chloro-phenoxyacetic acid) has been used satisfactorily by G. R. Jackman and M. A. H. Tincker to kill the green top-growth of common horsetail (*J. Roy. Hort. Soc.*, **74**, Pt. 8; Aug. 1949). Standard preparations of 'Agroxone' were used at 6 oz. per sq. yard. Applications in late May killed the green shoots for the season; but treatment in succeeding years is necessary to reduce the activity of the underground stems. Many garden plants, including dahlias, lupins and several conifers, were not damaged by the treatment. Others, for example, birch and *Cotoneaster* seedlings, were killed by it.

Principles of Radio Valves

A SERIES of attractive educational booklets, entitled "Electrons", is being published by the Edison Swan Electric Co., Ltd. (155 Charing Cross Road, London, W.C.2); the booklets, each in a different distinctive coloured cover, explain simply, by means of illustrations, diagrams and charts, the history and working principles of the radio valve. The first two of the series so far issued deal with the diode (red cover) and the triode (orange cover). Others to follow are: the screen grid (yellow), pentode (green), beam tetrode (blue), frequency changer (purple) and the multiple diode (grey). Copies of the booklets, as they are published, are available free to education authorities, radio societies and other bodies.

Pacific Science Association

FOLLOWING a decision made at the Seventh Pacific Science Congress held in New Zealand last year, a permanent secretariat of the Pacific Science Association has now been established in the Bernice P. Bishop Museum, Honolulu, for the purpose of maintaining the activities of the Association during the periods between congresses. Mr. Loring G. Hudson, recently information officer with the South Pacific Commission in New Caledonia, has been appointed executive secretary of the secretariat, and Miss Brenda Bishop, recently secretarial assistant for the Seventh Pacific Science Congress, is assistant secretary.

Public Health Laboratory Service Board

SINCE 1946, when the Public Health Laboratory Service was placed on a permanent basis, the Medical Research Council has continued to administer it as the agent for the Ministry of Health. It has been decided that the Council will, with certain reservations on matters of broad policy, now delegate this function to a Public Health Laboratory Service Board, composed in the first instance as follows: Dr. A. Landsborough Thomson (*chairman*), Prof. S. P. Bedson, Dr. C. Metcalfe Brown, Dr. A. N. Drury, Sir Percival Hartley, Sir W. Wilson Jameson (*ex officio*) and Dr. A. A. Miles, with Dr. W. H. Bradley as observer from the Ministry of Health.

New Electron Microscope at Glasgow

AN electron microscope has recently been installed in the Chemistry Building of the University of Glasgow. It is to be used, in conjunction with diffraction techniques, for a study of proteins and nucleo-proteins of biological interest. The instrument is under the care of Dr. I. M. Dawson, who has been appointed to an Imperial Chemical Industries fellowship. Dr. Dawson graduated at Glasgow in 1943, and then undertook researches there in photochemistry and X-ray crystallography. For the past two years he has been in charge of the electron microscopes at the National Institute for Medical Research, London.

Society for the Bibliography of Natural History

THE annual general meeting of the Society for the Bibliography of Natural History was held on April 24. At the meeting it was stated that a bibliography of the American naturalist C. J. Maynard, by C. F. Batchelder, is in the press, and it is intended to issue shortly the first of a series of facsimiles of rare or unpublished works. This will be part of a MS. notebook dealing with Linnæus's entomological lectures. Formal business was followed by a lecture on the "Medieval Bestiary", by Dr. Beatrice M. I. White. Officers of the Society were elected as follows: *President*, Dr. J. Ramsbottom; *Honorary Secretary and Treasurer*, Mr. A. C. Townsend; *Committee*, Mr. W. N. Edwards, Dr. F. C. Fraser, Mr. F. J. Griffin, Mr. Francis Hemming, Mr. W. T. Stearn and Mr. C. Kirke Swann.

Announcements

SIR HAROLD HARTLEY, president of the British Association, has had conferred on him the honorary degree of D.C.L. by the University of Oxford.

MR. H. HAYHURST has been elected president of the Manchester Literary and Philosophical Society on the retirement of Dr. E. J. F. James. Mr. Hayhurst will combine the two offices of president and honorary treasurer of the Society.

THE 1950 annual May lecture of the Institute of Metals will be given at the Royal Institution, Albemarle Street, London, W.1, on May 10, at 6 p.m., by Dr. H. Roxbee Cox, chief scientist, Ministry of Fuel and Power, his subject being "Industrial Gas Turbines". The lecture is open to the general public.

AT the anniversary meeting of the members of the Royal Institution held on May 1 the following officers were elected: *President*, Lord Brabazon of Tara; *Secretary*, Prof. A. O. Rankine; *Treasurer*, Dr. R. E. Slade.

THE Fifth International Congress of Scientific Research and the Social Fight against Cancer will take place at the Sorbonne, Paris, during July 15–22, under the presidency of Prof. A. Lacassagne, director of the Paris Radium Institute. At least fifty countries will be represented. Further particulars can be obtained from the secretary-general of the Congress, Prof. V. Le Lorier, 6 Avenue Marceau, Paris 8.

THE British Welding Research Association is offering a prize of £100 for an original unpublished paper on a research into welding or its application, carried out in Great Britain or Northern Ireland. Authors must be less than thirty-five years old and must limit their papers to less than five thousand words. The prize is financed by the British Oxygen Co. and was first offered in 1948. Papers must be submitted by September 30 to the Secretary, British Welding Research Association, 29 Park Crescent, London, W.1.

RADIOMIMETIC ACTION OF SULPHUR AND NITROGEN 'MUSTARDS' ON DEOXYRIBONUCLEIC ACID

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IT has been established that mustard gas and nitrogen mustards produce biological effects similar to those produced by X-rays on living tissues[1]. Among the effects which have been established are interference with mitosis, chromosome breakage[2], the induction of mutations[3], and carcinogenesis[4]. The action of X-rays and similar radiations on tissues and on isolated substances has been extensively studied, and it has been established that the action is for the most part indirect, the primary action of the radiations being the formation of free radicals such as H and OH, which react with the substrates present[5]. Recently, in a study of the action of X-rays on thymus nucleic acid, it was shown by Taylor, Greenstein and Hollaender[6] that this deoxyribonucleic acid, which may be taken as representative of the characteristic nucleic acid of the chromosomes, is degraded by the action of X-rays. It was found that it loses its structural viscosity; and that the loss of viscosity occurs mainly *after* the irradiation is completed. The loss of viscosity is accompanied by the loss of the original streaming birefringence.

The action of some nitrogen mustards on deoxyribonucleic acid was studied by Chanutin and Gjessing[7]. They observed a decrease in viscosity produced by the nitrogen mustards methyl *bis*-β-chlorethylamine $CH_3N(C_2H_4Cl)_2$, and the *tris*-β-chlorethylamine $N(C_2H_4Cl)_3$. However, their measurements did not permit an examination of the 'structural' viscosity (that is, the increase of apparent viscosity at low rates of flow) and were very limited in time.

We have determined the effect of mustard 'gas' and of the nitrogen mustard, $CH_3.N(C_2H_4Cl)_2$, on the structural viscosity of deoxyribonucleic acid derived from calf thymus. We used a viscometer of the Frampton type[8], in which the 'relative' viscosity is obtained as a function of the pressure producing flow. Fig. 1 shows a series of curves obtained during the reaction between the nucleic acid and the mustard in 0·1 *N* sodium bicarbonate. The nucleic acid shows originally a marked structural viscosity in that its relative viscosity rises rapidly with decreasing rate of flow. This structural component gradually disappears after the addition of the mustard, and the final result is a 'normal' liquid (that is, the viscosity is independent of the pressure) with a relative viscosity not much different from that of water and showing no streaming birefringence. Similar curves were obtained in a phosphate buffer at *p*H 7, in which, at the same concentration of the nitrogen mustard, the reaction is rather slower. The sulphur mustard, β-dichlor-ethyl sulphide, which was added to the nucleic acid solution dissolved in a little alcohol, produces a similar effect. The initial rate is rather less than that of the nitrogen mustard under similar conditions, but the final effect is much the same. Control experiments showed that the characteristics of the nucleic acid remained unchanged in the solutions employed over the period of the experiments.

These results are strikingly similar to the after-effects of radiation, which we have confirmed. Fig. 2 shows the change with time of the viscosity at a constant pressure of 60 cm. head of water, observed after the addition to the nucleic acid of the 'mustards' under various conditions and after irradiation with 22,000 r. of X-rays. The decrease in relative viscosity observed at this pressure is due mainly to decrease of the 'structural' component of the viscosity. There seems no doubt that there is a close similarity between the action of the mustards and of the X-rays on the nucleic acid. The monochlorotrialkylamine, $(CH_3)_2N.C_2H_4Cl$, had, however, practically no effect on the viscosity under similar conditions. This is in keeping with the observations that the monochlorotrialkylamines are relatively inactive biologically, and that usually two or more chloroethyl groups are required to give rise to the biological effects.

It has been noted by Boyland[9] that in mice and rats a dose of 1 mgm./kgm. of *bis*-β-chlorethylamine produces similar effects to a total body irradiation of 300–400 r. It may be significant that this ratio is of the same order as that observed (with much greater doses) in the relative action of the mustard and of X-rays in these experiments. It is found (Fig. 2) that a dose of 22,000 r. produces an effect which is

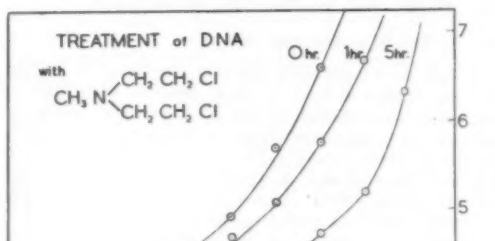

Fig. 1

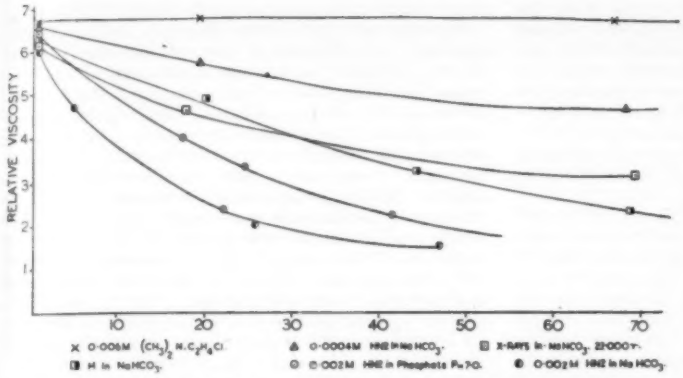

Fig. 2. Change of viscosity of nucleic acid after irradiation and after treatment with mustards

quantitatively similar to that of 2 mgm. of *bis*-β-chlorethylamine to 30 c.c. of 0·1 per cent nucleic acid, that is, sixty times as much radiation produces approximately the same effect as a total concentration of *bis*-β-chlorethylamine which is approximately sixty-six times that in the animal experiments.

It is the generally accepted view that the effects of X-rays on substances dissolved in aqueous solution are not produced by a direct action, but through the initial formation of short-lived radicals, such as H and OH, which react with the solutes[5]. Since the changes in the nucleic acid solutions continue when the irradiation is stopped, it seems that there must be two stages in the action : (1) the immediate action of the X-rays ; (2) a subsequent change which continues after the irradiation has stopped. There appear to be at least three ways in which the slow change (2) could be interpreted : (*a*) the radicals produced by the radiation initiate a chain reaction, which results in the degradation of the nucleic acid ; (*b*) the radicals react with nucleic acid forming an unstable intermediate product which slowly changes into the non-viscous final product ; (*c*) the radicals are transformed into a long-lived reactive agent such as hydrogen peroxide.

The experiments so far reported do not permit a clear distinction between these possibilities. There are many points in the nucleic acid molecule at which the OH radical could react ; for example, it could attach itself to the phosphoryl [:PO.OH] group so as to convert it into the per-phosphoryl group [:PO.OOH], which may be expected by analogy with perphosphates to be comparatively stable. (We are indebted to Dr. B. E. Conway for this suggestion.) A nucleic acid chain containing such groups could nevertheless undergo internal transformations which might result in its degradation.

It has been observed by G. Oster that the addition of reducing agents like cysteine and ascorbic acid inhibit the slow change[10]. We have found that the addition of cysteine to about the same amount as the nitrogen mustard *bis*-β-chlorethylamine prevents the change of viscosity normally brought about by the latter. This is perhaps to be regarded as an additional analogy between the action of X-rays and that of *bis*-β-chlorethylamine ; but it was found that in the presence of an excess of *bis*-β-chlorethylamine the action proceeded normally, so that the cysteine must perhaps be regarded as a competitor which reacts with and removes equimolecular quantities of the mustard.

According to Taylor, Greenstein and Holtaender[6], hydrogen peroxide at concentrations between 10^{-4} and 10^{-6} *M* has no effect on the viscosity of aqueous solutions of thymus nucleic acid. With our preparation we have found that hydrogen peroxide at a concentration of 0·001 *M* produces a change of viscosity of about the same order as that produced by the X-rays (22,000 r.) at *p*H 6–7. It is well known that hydrogen peroxide is formed on irradiation with X-rays in aqueous solutions containing dissolved oxygen[11]. It is therefore possible that at least part of the action of X-rays in oxygen-containing solutions is due to the action of hydrogen peroxide. This matter is being further investigated.

The fact that the dichloroalkylo 'mustards' produce similar changes in the nucleic acid would appear to weigh against the chain-reaction hypothesis. It is unlikely that they would form free radicals by their reaction with water or with the nucleic acid. On the other hand, nucleic acid contains numerous groups with which the active chloro-groups can react, through the formation of ester and similar linkages. It is significant that the compounds of this type which possess radiomimetic properties, as judged by the biological evidence, and the experiments reported here, have two or three reactive chloro-groups[12]. With these compounds, there will still remain after the reaction of one —Cl group with the nucleic acid a reactive centre in the molecule, which provides the possibility of a further reaction. Thus the initial combination of a molecule of this kind with the nucleic acid can be followed by a second reaction, either within the same or on another nucleic acid molecule, a situation similar in some respects to that which follows exposure to X-rays. It might be argued that the absence of an effect produced by the mono-chloro compound is due to its non-reaction with the nucleic acid under the conditions of the experiment. We have at present no direct analytical evidence that the mono-chloro compound did, in fact, react ; but we have found that a nucleic acid which has been treated with the mono-chloro compound reacts more slowly with *bis*-β-chlorethylamine than untreated nucleic acid, so that it has been changed in some way.

The suggestion made here as to the role of two reactive groups in the reactive molecules is in some respects similar to that made by Goldacre, Loveless and Ross[13], namely, that the two groups by reacting "at two distinct points, lying either on a single surface or fibres", act as cross-linking agents between nucleo protein or protein molecules. In this connexion it may be noted that Elmore *et al.*[14] studied the action of sulphur mustard on thymonucleic acid and isolated two products, one of which was more viscous than the original nucleic acid, and they showed that in this compound *two* phosphoryl groups were blocked for each molecule of mustard reacting. They regarded this as evidence of cross-linkage between the nucleic acid molecules. The conditions of the reaction were not, however, very closely com-

parable with the present experiments, in that the reaction was carried out in a much more concentrated solution, with an excess of the mustard, in 35 per cent alcohol. The decrease in viscosity of nucleic acid, and the loss of the structural component of the viscosity, which we have observed, would appear to indicate a depolymerization rather than an increase of molecular weight. Ultracentrifuge and diffusion measurements to establish this point are under way and will be reported later.

It must be remembered that nucleic acid employed in these experiments is a degradation product of the thymus nucleoproteins, and there is no sound reason for expecting that the reaction of the mustards with this product will necessarily closely parallel their action *in vivo*. Nevertheless, the fact that X-rays and the radiomimetic chemicals produce similar physical changes in the nucleic acids encourages a closer examination of the reaction products and of the mechanism of the reactions.

This investigation has been assisted by grants to the Royal Cancer Hospital from the British Empire Cancer Campaign, the Jane Coffin Childs Fund for Medical Research, the Anna Fuller Fund and the Division of Research Grants of the U.S. Public Health Service. We are indebted to Dr. L. F. Lamerton, of the Physics Department of the Royal Cancer Hospital, for carrying out the irradiations.

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γ-AMINO-BUTYRIC ACID AND β-ALANINE IN PLANT TISSUES

Amino-Acids of the Apple Fruit

IN the early stages of applying the technique of paper partition chromatography along the lines suggested for the potato tuber by Dent, Stepka and Steward[1] to a re-examination of the amino-acid metabolism of the apple, it became evident that the most concentrated extracts of the fruit that could be obtained by evaporative methods still did not contain a sufficiently high concentration of amino-acids for the production of a satisfactory chromatogram. With young fruits (average weight, 10 gm.) which contain a relatively high content of nitrogen, the only amino-acid to appear was asparagine, which comprises about 70 per cent of the total soluble nitrogen in such fruits[2].

The factor which militates against further concentration by evaporation is the high sugar content of the fruit. Bramley's Seedling apples at the stage of development used—a stage at which the sugar–nitrogen ratio is almost at its lowest—contain, on a percentage fresh-weight basis, about 1,500 mgm. total sugar and only about 30 mgm. of total soluble nitrogen. A satisfactory way of resolving this difficulty was found in the use of the cation exchange resin 'Zeo-Karb 215' as an absorptive filter for the amino-acids[3]. In some cases the apple-pulp extracts[3] were given a preliminary treatment with charcoal, treated as described by Partridge[3], to remove aromatic amino-acids and troublesome colouring matter, before being passed down the 'Zeo-Karb' column. Sugars were completely washed from the column with distilled water (*p*H 5·5) and the amino-acids were then eluted with ammonia and finally soda (for arginine). The *p*H of the eluate was followed and several 'cuts' were taken; but no attempt at critical separations was made at this stage. The three or four amino-acid extracts were evaporated to small bulk *in vacuo*, and the resultant concentrated solutions were chromatographed on paper separately in the usual way. For the present purpose the results for each fraction will not be considered separately.

Two-dimensional chromatograms were found, after preliminary trials, to be somewhat unsatisfactory and, in view of the small number of acids present, unnecessary.

Spots of the concentrated eluate were run in phenol–ammonia, *n*-butanol–acetic acid (which gave the best single-solvent separation), *iso*butyric acid and collidine, against a range of pure amino-acids singly and in mixtures. Where specific microchemical tests are available, these were applied to 'paper-travelled' spots of the unknown and 'suspected' pure acids to obtain confirmatory evidence. The nature of these tests are indicated in column 3 of the accompanying table. In this table are shown the amino-acids definitely identified (+) and those probably present (?) in the pulp of Bramley's Seedling apples at two stages of development, namely, at average weights of 10 gm. (pick I) and 100 gm. (pick VI).

AMINO-ACIDS PRESENT IN THE PULP OF BRAMLEY'S SEEDLING APPLES

Amino-acid	Pick I (June 8)	Pick VI (Aug. 20)	Confirmatory test
Glycine	?	+	Phenol + sodium hypochlorite
Valine	+	–	
Serine	?	–	
*Iso*leucine	+	+	
Phenyl alanine	+	+	
Tryptophane	+	+	*p*-Dimethylamino benzaldehyde in acid
β-Alanine	+	+	
γ-Amino butyric acid	+	+	
Aspartic acid	+	+	
Glutamic acid	+	–	
Asparagine	+	+	
Arginine	–	+	α-Naphthol and sodium hypochlorite
Proline	+	–	Sodium nitroprusside + acetaldehyde + sodium carbonate

At both stages of development, asparagine and γ-amino-butyric acid were in considerable excess of any of the other acids. It is possible that β-alanine may be confused with citrulline; but the specific test for citrulline mentioned by Dent[4] gave a negative result when applied to our chromatograms. In addition to the above, there was an unidentified amino-acid having a very low R_F in all solvents and running in butanol–acetic acid and *iso*butyric acid just ahead of the cystine pair, which themselves

appear to be absent from our extracts. The absence of tyrosine is unexpected, but not that of glutamine[5].

It is believed that this is the first time that the presence of β-alanine and γ-amino-butyric acid has been reported in fruits. The large amount of γ-amino-butyric acid is of particular interest, since in previous work on the nitrogen metabolism of the apple[3] a high proportion of the soluble nitrogen appeared in the 'rest-nitrogen' fraction, that is, it was not estimated either as 'amide-nitrogen' or 'amino-nitrogen'; in addition, the apple was found to be poor in 'basic-nitrogen'[5]. It is probable that a considerable proportion of the 'rest-nitrogen' may have arisen from γ-amino-butyric acid since, under the conditions of the usual Van Slyke α-amino-nitrogen determination employed in the work mentioned, only a portion of the γ-amino-group would react[6].

In conclusion, we should like to express our thanks to Dr. C. E. Dent for a sample of pure γ-amino-butyric acid.

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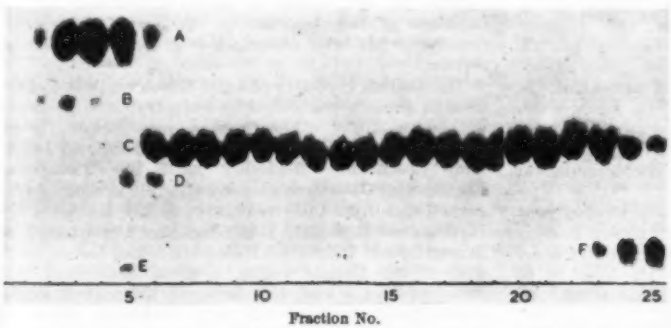
Isolation of γ-Amino-butyric Acid from Beetroot (*Beta vulgaris*)

A general method for the isolation of amino-acids and organic bases by displacement chromatography using synthetic cation exchange resins has recently been described[1-3]. The immediate object of the present work, which was readily achieved, was to apply this technique to the preparation of glutamine in quantity from the aqueous extract of red beet-roots. During the course of the fractionation, a ninhydrin-reacting substance was observed which was identified by paper chromatography as γ-amino-butyric acid. The primary fractionation on 'Zeo-Karb 215'[3] yielded a range of common amino-acids which appeared in the effluent in the order expected from the experiments on protein hydrolysates. In addition, paper chromatograms[4] carried out on the successive fractions of the effluent showed the presence of a large amount of glutamine and smaller amounts of asparagine, γ-amino-butyric acid and β-alanine. Ninhydrin spots due to γ-amino-butyric acid have been observed on paper chromatograms of extracts of a number of plant and animal tissues[6-7], but the direct isolation of the amino-acid by conventional methods has proved difficult.

In all, fifty-eight fractions, each of 250 ml., were taken during the primary fractionation. Fractions 1–6 contained aspartic acid, glutamic acid, serine and threonine; 7–16 included glutamic acid, asparagine, glutamine and alanine; 17–34 contained almost entirely glutamine, but with small amounts of alanine (these two substances form a mixed band under the conditions employed); 35–48 showed diminishing amounts of glutamine and contained a basic substance which did not react with ninhydrin. This base gave a brown coloration when sprayed with iodine reagent[8] and has now been isolated; but its identity remains to be established. Fractions 49–56 contained small amounts of valine, cystine, phenyl-alanine, histidine and β-alanine, together with larger amounts of leucine and γ-amino-butyric acid. The position of γ-amino-butyric acid in the effluent fractions showed that it was more strongly adsorbed than leucine but less so than histidine.

For the isolation of the γ-amino-butyric acid, fractions 52–54 (750 ml.) were bulked and evaporated under reduced pressure. Some leucine and cystine were removed in a series of small crops, but the concentrated mother liquor could not be induced to crystallize from water, ethanol or acetone. A further purification was achieved by fractionation through a smaller 'Zeo-Karb' column containing 50 gm. of resin using 0·15 *N* aqueous ammonia as the displacement developer. The fractions collected, each of 25 ml., were analysed on a paper chromatogram using butanol – 10 per cent acetic acid. The accompanying illustration shows a reflex photograph of the ninhydrin-developed paper chromatogram. Fractions 8–22, which showed γ-amino-butyric acid only, were bulked and evaporated under reduced pressure to about 5 ml. The syrup obtained yielded needle-shaped crystals upon the gradual addition of ethanol to a final volume of 60 ml. 2·54 gm. was obtained, and a further crop, after evaporation, yielded 0·47 gm. The crystalline material, when run side by side with synthetic γ-amino-butyric acid (kindly provided by Dr. C. E. Dent), gave identical R_F values in phenol – ammonia, butanol – 10 per cent acetic acid mixture and collidine. It melted with decomposition at 193° (uncorrected). References in the literature[9,10] give a range of values from 190° to 200° (found: C, 47·0; H, 9·0; N, 13·8. Calc. for $C_4H_9O_2N$: C, 46·6; H, 8·7; N, 13·6 per cent). The *p*-toluene-sulphonyl derivative melted at 135–136° (uncorrected). Thomas and Goerne[11] reported a melting point 135° for this substance. The yield was 3·01 gm. of γ-amino-butyric acid isolated from 40 lb. of trimmed beetroots representing 0·016 per cent of the fresh weight. Allowing for small amounts remaining in the mixed fractions, the minimum amount



A paper chromatogram of successive effluent fractions (reflex photograph). (*A*) leucine(s); (*B*) valine; (*C*) γ-amino-butyric acid; (*D*) β-alanine; (*E*) cystine; (*F*) histidine

present in the fresh beetroot was probably about 0·02 per cent.

I should like to express my gratitude to Dr. S. M. Partridge for his advice and constant encouragement, and to Dr. C. E. Dent for a gift of γ-aminobutyric acid.

The work described in both these papers was carried out as part of the programme of the Food Investigation Organisation of the Department of Scientific and Industrial Research.

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Nov. 22.

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HYDROMECHANICS AND HYDRAULICS RESEARCH IN GREAT BRITAIN

AT the annual general meeting of the British Hydromechanics Research Association, Sir John Anderson was re-elected president, and a council of eighteen elected with Mr. G. A. Wauchope (Gwynne Pumps, Ltd.) as chairman and Mr. E. Bruce Ball (Glenfield and Kennedy, Ltd.) deputy chairman. The chairman reported to the meeting that during the past year it had become evident that, in addition to the facilities at the proposed laboratory at East Kilbride of the Mechanical Engineering Research Organisation, it was desirable for the Association to have its own research station in which individual research problems could be tackled. This would in no way compete with the government laboratory at East Kilbride, which would probably concentrate on long-term generic problems.

In the second annual report of the council, covering the period October 1948–September 1949*, it is stated that the negotiations with the Harlow Development Corporation for the building of the research laboratory and offices of the Association, about 12,000 sq. ft. in area, have been successful. It is expected that the laboratory will be among the first buildings to be erected in the new satellite town of Harlow, Essex, and it is hoped that the laboratory will be completed this year. Housing accommodation for the staff will be available.

During the year under review, considerable progress was made in building up an industrial income sufficient to earn a government grant; but uncertainty existed until the very end of the year as to whether the minimum required income would be attained. In consequence, though slightly more than £16,500 was finally obtained, development was somewhat retarded. It is considered that, in order to cover adequately the extensive field of interest of the Association, a team of twenty qualified research engineers will be required eventually with a corresponding expenditure of some £60,000 a year. At present the staff consists of the director of research, Mr. L. E. Prosser, and four research engineers and a library assistant.

A list of the ordinary, and associate, members of the Association is given in the annual report. It comprises thirty-six manufacturing and user members, and thirty-six consultants and academic workers. The corresponding figures for the previous year were thirty-one in each group. The very small increase in membership is to be deplored and indicates that, while industrial firms admit that active research is essential to progress, they are reluctant to foster it. Tribute is paid to the British Electricity Authority, which is giving substantial financial support to the Association.

Two visitors, Brigadier R. A. Bagnold and Prof. L. Rosenhead, entrusted to make an impartial appraisal of the scientific work of the Association, were appointed by the Department of Scientific and Industrial Research, and close liaison has been maintained with the Mechanical Engineering Research Organisation, the Admiralty, and the Information Department of the Ministry of Supply. Six research committees, on which some forty well-known hydraulic experts serve, and an information service advisory committee assist the director of research in his work, and details of some of the sixty technical problems which have been dealt with during the year are given in the report. Subjects on which work is in progress include: pressure surges in pipe lines, design of suction inlet sumps of centrifugal pumps, valve behaviour in reciprocating pumps, and seals and glands. One research committee has been investigating pipe friction formulæ and has arrived at conclusions which probably will be recommended shortly for general acceptance. Quantitative information concerning friction at pipe-line fittings can be of considerable economic importance, as it is not always appreciated how much loss can be caused by unsuitable fittings.

Since November 1948, the Association has issued at regular bi-monthly intervals an abstract bulletin, containing sixty to seventy informative abstracts of current literature on hydraulics, details of translations of foreign papers available through either the Association or other organisations, and information concerning the technical notes circulated to the members of the Association. A list of these notes together with brief abstracts is given in the annual report.

The British Hydromechanics Research Association, though directly controlled by industry, is, in common with some forty other research associations, under the ægis of the Department of Scientific and Industrial Research. Together with the Mechanical Engineering Research Organisation, it is mainly concerned with hydraulic machinery and equipment. The Hydraulics Research Board of the Department of Scientific and Industrial Research, under the directorship of Sir Claude Inglis, is more concerned with basic problems and has devoted its efforts to the investigation of tidal and river problems, though it is considerably hampered by lack of a permanent laboratory and adequate facilities.

* British Hydromechanics Research Association. Second Annual Report, October 1948 to September 1949. Pp. 24. (79 Petty France, London, S.W.1.)

Several European countries possess well-established hydraulic laboratories; but the study of hydraulics in Great Britain is comparatively recent. It is interesting, therefore, to learn that the Hydraulics Research Board, after a long search for a suitable site for a permanent station, has at last satisfactorily concluded negotiations for a site at Howbery Park, near Wallingford. It comprises an estate of ninety acres stretching along the Oxford bank of the River Thames upstream of Wallingford Bridge. The large manor house on the estate will be used for offices, library, canteen, and small sedimentation, chemical and photographic laboratories of the proposed Hydraulics Research Station. A wave tank in the open, 250 ft. long by 160 ft. wide and 2 ft. 6 in. deep, in which coast erosion and wave problems will be studied, and a 5-ft. flume and a 12-ft. wide channel, each 350 ft. long, in which the effect of charge, grade, specific gravity and shape of particles on slope and shape will be investigated, are to be constructed. These will be followed by the construction of several demonstration models, workshops, laboratories and the first part of a main building. Detailed designs for these have already been prepared by the Ministry of Works, and work is to be commenced early this year.

Until the new Research Station is ready for use, probably in 1951, the research work of the Board is being continued at Teddington at the National Physical Laboratory, and in several suitable university departments, notably that at the Imperial College of Science and Technology, London. A central office for the Board has been set up in Rex House, Lower Regent Street, London, and a small block of offices has been made available at the National Physical Laboratory. The number of staff of the organisation has now been increased to twenty-five non-industrial and four industrial members, and preliminary work has been commenced on the setting up of a library and instituting exchange arrangements with corresponding organisations abroad.

Details of the research work carried out or in progress make interesting and impressive reading. Experiments have been conducted in the Tidal Model Laboratory of the National Physical Laboratory on the models of the River Forth and the River Wyre. For the River Forth the major questions being investigated are the effect on the regime of the estuary of constructing embankments, piers of the proposed Forth Bridge, dredging round the Beamer Rock, removing the Beamer Rock, and extending the South Arm of Rosyth Dockyard. Echo-sounding surveys of the estuary of the River Wyre have shown that the river course has been moving fairly rapidly, the deep channel opposite Burn Naze having swung across the estuary from the west bank to the east bank in some six years. The River Wyre model as originally designed did not reproduce the rapid changes observed in the estuary, and changes in design are under consideration.

A new tilting flume, some 35 ft. long, recently obtained from the River Trent Catchment Board and installed at Teddington, is likely to prove a valuable piece of equipment. Improvements in the designs of instruments for recording water-levels, wave heights and small velocities have been made. A new flow-meter recently designed by one of the staff can be used to record velocities and directions of flow at various heights above the bed, from which the velocity gradient and hence, by extrapolation, the approximate bed-velocity can be determined. A new type of pneumatic wave-generator has been developed consisting of an air chamber in which the pressure is varied to create tidal flow. Wave-pressure research under the direction of Prof. C. M. White at the Imperial College of Science and Technology has continued; and investigation of the movement of solid particles in water, which presents extraordinary practical difficulties in its solution, is being tackled in its more empirical aspect at Teddington, and in its more fundamental aspect by Brigadier R. A. Bagnold, also at the Imperial College.

The Director and staff have visited many hydraulic laboratories abroad and have attended international conferences. Advice and information have been given to many engineering firms, catchment boards and government departments, and there is abundant evidence that the facilities of the research organisation, when fully developed, will be in great demand.

CALCULATION OF PERTURBATIONS OF COMETS AND MINOR PLANETS

A PAPER by G. Merton on "A Modification of the Perturbations-of-Elements Method"[1] suggests a new method for computing the perturbations of a comet or minor planet. For many years the Computing Section of the British Astronomical Association has used Crommelin's tables[2], which cover a range in eccentricity of 0·37–0·84 and can be applied for nearly all periodic comets. The mean anomaly in these tables is divided into equal intervals of 7½°, except for perturbations caused by the earth, when the intervals are 1°; in most cases their use proved satisfactory, but for large perturbations they sometimes gave results that were not very reliable. This was to be expected from a method which assumed that one set of elements could be used over long intervals to represent the actual circumstances in the perturbed orbits, an assumption which, in some cases, was not justified.

Merton previously used a different method, first devised by Oppolzer, in computing the perturbations of Comet Grigg-Skjellerup, the divisions being made in equal intervals of 20° of eccentric anomaly, and very satisfactory results were obtained, although he concluded that the mean anomaly division of the orbit was preferable. In his recent paper Merton suggests that a rigorous form of the mean-anomaly method should be used which will be accurate over any period, and this can be achieved by making time one of the perturbed elements and the mean anomaly the independent variable. J. Bauschinger has given the standard formulæ, using time as the independent variable, in "Die Bahnbestimmung der Himmelskörper", and to adapt these to the new method suggested implies multiplying each differential equation for the various elements by dt/dM and integrating with respect to M instead of to t.

The whole scheme is fully developed, and the formulæ required for the computations are given with a fictitious example from a comet of period 7 years, eccentricity 0·6, inclination to the ecliptic zero, and the closest approach to Jupiter, the only planet considered as the perturbing body, at a date when the latitude of Jupiter was zero. It is interesting to know that in this particular case the 7½° interval of

Crommelin's tables gives results which are sufficiently accurate; but the example, while illustrating the application of the new method, is not a fortunate selection to show the weakness of the old method in which time is the independent variable. Merton shows how the tables of Crommelin for comets, and also those of Stracke for approximate minor-planet work, can be easily modified to suit the proposed new method. But, in addition, new arrangements in the old tables are essential; in particular, smaller intervals around perihelion are necessary, and 3° intervals here would suffice for all practical purposes.

Various other modifications are suggested, and possibly the new scheme will be tried as soon as practicable by the Computing Section of the British Astronomical Association.

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[2] Crommelin, A. C. D., *Mem. Roy. Astro. Soc.*, **64**, Pt. 5 (1929).

ROTHAMSTED EXPERIMENTAL STATION

REPORT FOR 1948

THE publication of the report for 1948 of the Rothamsted Experimental Station* includes brief reports from the thirteen Departments, namely, Physics, Chemistry, Pedology, Soil Microbiology, Botany, Crop Physiology, Statistics, Plant Pathology, Biochemistry, Nematology, Entomology, Bees, and Insecticides and Fungicides; these are listed in order to give some indication of the comprehensive nature of the report. An account of the Rothamsted field experiments during 1948 is also given, as well as a report by the Farm Director. A progress report of the Soil Survey of England and Wales, which is now directed from Rothamsted, is also included, and, finally, a review of work on the serological reactions of plant viruses. All the investigations are concerned with soils and crops and with some of the diseases and insect pests which limit growth. Some of the work is directly concerned with problems that are causing concern to growers to-day, for example, eelworm and viruses of potatoes and of sugar beet. Taken as a whole, however, the research work is not necessarily directed to solving problems of immediate interest; but is of the fundamental kind that will enable a rational and more rapid approach to be made to the solution of many of the growers' difficulties in the future. There is a wide choice of important research, and the significance attached to the various lines of work must depend mostly on the reader's particular interest.

In the Physics Department considerable work has been done in the past few years on evaporation, and an interesting application of some of this is noted in a joint experiment on the spray type of irrigation carried out on the farm of a well-known grower in Surrey. A small weather station was set up on the field, and, from records sent weekly to Rothamsted, the transpiration was estimated and the trend of the estimated soil moisture followed during the season. The soil moisture deficit as calculated from meteorological data agreed very well with that obtained from soil samples. A report of these experiments on controlled irrigation is included with the section on field experiments. A large increase in sugar beet resulted from irrigation in the dry summer of 1947; but there was very little difference in yield in the following year, when the summer was wet. Perhaps the most surprising result was that the addition of potassium nitrate to the irrigation water made no difference to the weight of crops.

The work of the Chemistry Department, on sewage sludge and composts, summarizing the results of more than a hundred field experiments, cannot fail to interest a wide circle, especially in view of the controversial subject of 'organics'. It is concluded that the crop-producing power of sludges taken from drying-beds is much less than that of equal weight of farmyard manure. Its physical effects on the soil are less pronounced.

It is interesting to note the widening scope of the Statistical Department, which now includes the organisation, supervision and analysis of agricultural surveys. Three surveys are included, one of fertilizer practice, one of main-crop potatoes and one of methods of milk production. The potato survey includes a test of methods of forecasting the yield, a month or so before lifting, by taking weighed samples of the growing crop. A text-book on "Sampling Methods in Censuses and Surveys" was being prepared for publication.

The new Department of Nematology was set up at Rothamsted during the year. It is concerned with eelworm parasites of plants. There are those that attack stem and shoot, as in the chrysanthemum eelworm, and those that attack the roots, for example, the potato and the sugar-beet eelworm. Work on the latter type has been mainly concerned in bringing to completion the Agricultural Research Council field trials on the nematocidal effects of D-D mixture. At the most responsive of sites, yield, kill and final eelworm-population were all roughly proportional to the rate of application. In some cases, however, an initial reduction in eelworm population was more than made good during the growth of the subsequent crop, when the eelworm multiplied at an accelerated rate.

The long-term rotation and ley-arable experiments will throw light on many aspects of systems of cropping that are practised to-day, and also on new rotations that some progressive farmers may be contemplating. In the Woburn ley-arable experiments begun in 1938, the object is to test the effects on soil fertility of the different systems of cropping. Two similar but more comprehensive experiments have now begun at Rothamsted whereby it will also be possible to compare the two systems—permanent grass plus permanent arable against alternating grass and arable. There will be a direct comparison of the output from permanent grass as compared with temporary grass under the same conditions. There is so much taken for granted in making such comparisons to-day that many will welcome the planning of these experiments at Rothamsted, where the necessary resources exist for obtaining reliable data.

Reference was made above to the possibility that some progressive farmers may be contemplating a change to a new cropping-system. The Rothamsted data on the restricted benefits of inter-row cultivations, the advent of the new weed-killers, and the intensive management of leys will probably lead many to break away from the traditional rotational system that has held sway for two hundred years. Before making the decision, many will appreciate much of the accurate information that will become available from these experiments.

* Rothamsted Experimental Station, Harpenden. Report for 1948. Pp. 151. (Harpenden: Rothamsted Experimental Station, 1949.) 5s.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Photographic Plate Evidence for the γp-Reaction in Be^9

THE reaction Be^9 γp Li^8 was first observed by Ogle, Brown and Conklin[1] in irradiations made with a 22-MeV. betatron employing counter techniques to observe the delayed β-particle emission from the lithium-8 and the α-particles from the beryllium-8. More recently, Baldwin[2] has reported that the relative cross-section shows a maximum at 32-MeV. γ-ray energy. Because lithium-8 is unstable, the reaction proceeds by three stages :

$$Be^9\ \gamma p\ Li^8, \quad Li^8 \xrightarrow[0{\cdot}88\ \text{sec.}]{\beta} Be^{8*}, \quad Be^{8*} \rightarrow 2He^4,$$

and leads to the production of a three-particle star in photographic emulsions which are not electron-sensitive. Such stars have been observed in beryllium-loaded Ilford plates type C_2 and E_1 irradiated with 24-MeV. γ-rays from the synchrotron at this Establishment. Of the twenty-one events observed to date, that of highest energy is shown in the photomicrograph of Fig. 1, where the proton, recoiling lithium-8 nucleus and the two α-particles from the break-up of the beryllium-8* nucleus are visible. The distribution in range of the protons emitted is shown in Fig. 2, from which it will be seen that in about half the cases the proton escapes from the emulsion. As there are insufficient events to enable escape corrections to be applied, results on the variation of cross-section with energy cannot be given at this stage. However, the mean value of the cross-section over the energy-range 18–24 MeV. can be calculated from the results and is $(2{\cdot}5 \pm 1{\cdot}0) \times 10^{-28}$ cm.². In deriving this value, the density of beryllium in the emulsion was taken as 0·0079 gm./c.c. (a figure supplied by Messrs. Ilford, Ltd.), the theoretical spectrum calculated by Heitler[3] was assumed and γ-ray intensity measurements with a thick-walled ionization chamber were employed.

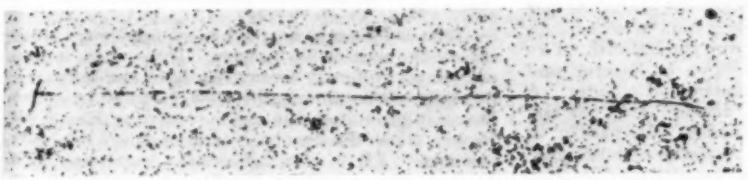

Fig. 1. Reaction Be^9 γp Li^8. The proton, recoiling lithium-8 nucleus and two low-energy α-particles from the break-up of the beryllium-8 nucleus are visible. Observer : Miss K. B. Treacy

The proton distribution indicates a threshold for the reaction at about 18 MeV., a result in agreement with the figure 18 ± 1 MeV. given by Ogle and Conklin[1], compared with 16·86 MeV. calculated from the masses[4].

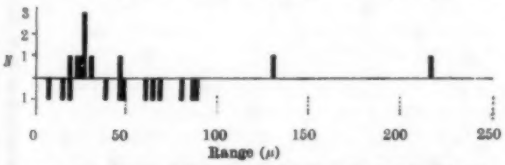

Fig. 2. Number-range histogram. Events above line are wholly contained in the emulsion. Below the line the proton leaves the emulsion at the range indicated

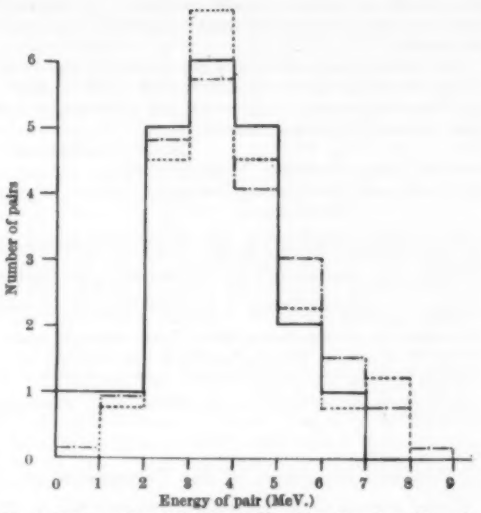

Fig. 3. Energy release in break-up of beryllium-8 nucleus. Present experiment full lines, Christy *et al.* broken lines, Franzinetti and Payne dotted lines. All normalized to correspond to the same number of events

A histogram of the total energy release in the break-up of the Be^{8*} nucleus into two α-particles is given in Fig. 3 for the twenty-one events, and indicates a broad level somewhat higher than 3 MeV. This agrees with that given by Franzinetti and Payne[5] obtained on lithium-8 nuclei emitted in cosmic ray stars and Christy *et al.*[6] on lithium-8 nuclei obtained from the Li^7 dp reaction, histograms of which are shown in the same diagram.

Evidence has also been sought for the $Be^9\gamma n$ reaction. Pairs of α-particles would be expected to diverge from a point as a result of the two-stage process :

$$Be^9\ \gamma n\ Be^8, \quad Be^8 \rightarrow 2He^4,$$

the beryllium-8 nucleus being possibly left in an excited state. At the higher γ-ray energies the recoiling beryllium-8 nucleus would have an energy such that subsequent break-up into two α-particles would produce tracks long enough for observation even if it were formed in the ground-state. More than sixty V-shaped pairs of tracks have been observed ; but analysis to determine the energy release from the beryllium-8 disintegration by a method already described[7] gives no indication of a level structure for the beryllium-8 nucleus. It is probable, therefore, that these α-particle pairs do not result from the γn-reaction, since a background of such events would be expected due to the collision of single α-particles (present in the emulsion from $\gamma\alpha$-reactions) with silver and bromine nuclei.

If the reaction proceeds at high energies by the alternative two-stage process :

$$Be^9 + h\nu \rightarrow He^4 + He^5, \quad He^5 \rightarrow He^4 + n^1,$$

there would be much greater energy in the α-particle

arms, and such events would be difficult to identify in the present experiment where thin emulsions were employed.

The helpful co-operation of the Synchrotron Group of this Establishment is acknowledged both in effecting the irradiations and providing information on γ-ray intensity measurements.

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Jan. 25.

[1] Ogle, Brown, and Conklin, *Phys. Rev.*, **71**, 378 (1947). Conklin and Ogle, *Phys. Rev.*, **73**, 648A (1948).
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[4] Bethe, H., "Elementary Nuclear Theory" (John Wiley and Sons).
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[6] Christy, R. F., Cohen, E. R., Fowler, W. A., and Lauritsen, C. C., *Phys. Rev.*, **72**, 698 (1947).
[7] Goward, F. K., Titterton, E. W., and Wilkins, J. J., *Nature*, **164**, 661 (1949).

Possible Emission of the Dineutron in Fission

PROF. N. FEATHER[1] has suggested that, if the dineutron exists, it may be emitted in fission and therefore be present in pile radiations. Further, it is pointed out that bismuth-209 would be a suitable detector for the dineutron, in that it produces actinium C, which has an alpha activity of 2·16 min. half-life readily observable in the presence of other activities produced in the irradiation.

$$Bi^{209} + n_0^2 \rightarrow AcC^{211} \xrightarrow[\alpha]{2\cdot16\ \text{min.}} AcC'' \xrightarrow[\beta]{4\cdot8\ \text{min.}} Pb^{207}$$

$$Bi^{209} + n_0^1 \rightarrow RaE^{210} \xrightarrow[\beta]{5\ \text{days}} Po^{210} \xrightarrow[\alpha]{140\ \text{days}} Pb^{206}$$

We have carried out two 10-min. irradiations of bismuth in the Harwell pile at a point where the thermal neutron flux was about 10^{12} neutrons per sq. cm. per sec. and where only 2 mm. of aluminium separated a piece of uranium from the bismuth. Two minutes were required to remove a detector and insert it in an argon-filled proportional counter. A high-frequency amplifier and discriminator fed any alpha-particle pulses into a scaler. In neither of the irradiations was there any evidence of actinium C activity. The growth of the polonium activity from the radium E was observed over several days.

Let φ_2 be the flux of dineutrons at the detector and σ_2 the absorption cross-section of bismuth for them. Then

$$\varphi_2\sigma_2 = \frac{N_2}{N_1} \cdot \frac{R_1}{R_2} \cdot \varphi_1\sigma_1 \text{ (for a thick detector),}$$

where N_2, N_1 are the observed counting-rates of actinium C and polonium corrected to long irradiation time; R_2, R_1 are the ranges in bismuth of the actinium C and polonium alpha-particles; φ_1 is the thermal neutron flux at the detector and σ_1 the capture cross-section of bismuth for thermal neutrons. Using $\sigma_1 = 0{\cdot}015$ barn, we conclude from two irradiations that $\varphi_2\sigma_2 < 1{\cdot}5 \times 10^{-21}$ sec.$^{-1}$.

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[1] *Nature*, **162**, 213 (1948).

Crystal Structure of Boron Nitride

THE accepted structure of boron nitride, proposed by Hassel[1] and Brager[2,3], is that of graphite with the carbon atoms replaced by boron and nitrogen atoms, and is designated structure type *B*.12 in the "Strukturbericht"[4]. This structure does not give a good agreement between observed and calculated intensities; and as the original work is open to other criticisms, a complete redetermination has been carried out. It is concluded that the Hassel structure is incorrect.

Powder photographs of recrystallized boron nitride were obtained with copper and manganese *K*-radiation. Lines corresponding to twenty-eight diffraction planes were obtained, and these can all be indexed on the basis of a hexagonal unit cell with dimensions at 35·5° C. of $a = 2{\cdot}5038 \pm 0{\cdot}0001$ A., $c = 6{\cdot}660 \pm 0{\cdot}001$ A. Intensity measurements were made from the photographs using a calibrated microphotometer, and also with a Geiger counter spectrometer. From these data and the density[5] it can be readily shown that boron nitride has a layer structure with an interlayer spacing of $\frac{1}{2}c$; each layer consists of a flat network of B_3N_3 hexagons.

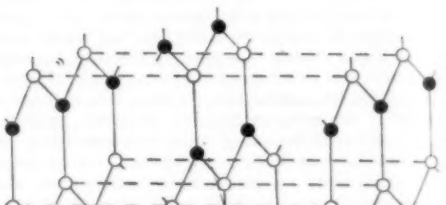

Fig. 1. Hassel structure of boron nitride
●, Boron atom; ○, nitrogen atom

These layers can be packed in four ways which satisfy the unit cell symmetry. Three of these ways are simply different arrangements of the boron and nitrogen atoms among the sites occupied by carbon atoms in graphite; the one favoured by Hassel and Brager, which fits the observed intensities least badly, is shown in Fig. 1. Fig. 2 shows the fourth way. This is a new type of packing, in which the hexagonal rings are packed directly on top of each other, the positions of the boron and nitrogen atoms being interchanged in adjacent layers. Comparison of the observed with the calculated intensities shows conclusively that boron nitride has this new type of packing, and not the graphite type.

The most striking consequence of this difference between boron nitride packing and graphite packing is

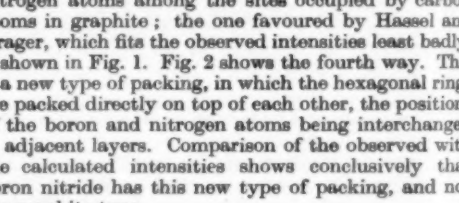

Fig. 2. New proposed structure of boron nitride

the difference between the relative intensities of the *hkl* lines when $h + 2k \neq 3n$. In boron nitride, these lines are strong when l is even, and weak when l is odd; whereas in graphite the reverse is true. This difference is readily visible on the powder photographs, and cannot be explained as a consequence of the difference of atomic scattering powers between boron and nitrogen atoms on one hand and carbon atoms on the other.

A full account of this work will appear elsewhere.

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Jan. 3.

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[2] Bragar, A., *Acta Physicochem. U.S.S.R.*, **7**, 699 (1937).
[3] Bragar, A., *Acta Physicochem. U.S.S.R.*, **11**, 617 (1939).
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Structure of cyclo-Octatetraene

THREE structures, namely, D_{4d}, D_4 and D_{2d} have been proposed for this substance, the first having all C—C bonds equivalent and the other two having single and double C—C bonds. On the basis of the Raman spectra and electron diffraction studies, Langseth and co-workers[1] favour the D_{4d} structure. Lippincott and Lord[2] are, however, inclined towards the D_4 structure due to the presence of frequency 1655 in the Raman spectrum which has been considered as a C=C frequency, whereas Fleet and co-workers[3] favour the D_{2d} structure on the basis of the infra-red studies. Both Langseth and Maccoll[4] have cited other evidence from the absorption and thermochemical data in favour of the resonating D_{4d} structure, and Kaufman[5] from X-ray studies in the solid and Coulson and Moffitt[6] from theoretical quantum mechanical treatment favour the D_{2d} structure.

Our analysis of the Raman and infra-red frequencies on the basis of the D_{2d} structure shows clearly that all infra-red frequencies must be Raman-active. There are two very strong infra-red frequencies, 802 and 675, and one medium strong at 2,970 cm.$^{-1}$ for which there are no corresponding Raman lines. Further, we need six polarized and twenty-one depolarized Raman frequencies for this structure, whereas the number of observed polarized lines is four and that of depolarized lines ten. While we are not inclined to attach great importance to the second argument, the first one appears to us to be decisive against this structure. In our opinion the best structure in agreement with the observed number of polarized and depolarized Raman lines is D_{4d}, which requires four polarized and eleven depolarized lines. Structure D_4, which requires six polarized and twenty-two depolarized Raman lines, however, cannot be ruled out on spectroscopic data because the observed polarized Raman frequencies 200, 877 and 1655 are found to represent correctly the ring frequencies for this structure.

We have calculated the frequencies for the D_{4d} structure taking the electron diffraction data of Langseth, namely, equivalent C—C bonds of 1·42 A., C—C—C angle of 120° and C—H bond of 1·09 A. The forty-two internal modes of the molecule are divided into seven classes: A_1 (4), A_2 (1), B_1 (3), B_2 (2), E_1 (5), E_2 (6) and E_3 (5), of which E_1, E_2, E_3 are doubly degenerate and A_1 totally symmetric. A_1, E_2, E_3 are active only in the Raman effect, B_1 and E_1 only in the infra-red, while A_2 and B_2 are forbidden in both. Using six force constants, namely, K_1 (C—C valence) = $5{\cdot}7 \times 10^5$, K_2 (C—H valence) = $4{\cdot}9 \times 10^5$ dynes/cm., K_3 (C—C—C deformation) = $-0{\cdot}27 \times 10^{-11}$, K_4 (C—C—H deformation) = $0{\cdot}3 \times 10^{-11}$, K_5 and K_6 (angular deformations produced by carbon and hydrogen atoms respectively going out of the C—C—C plane) = $0{\cdot}1 \times 10^{-11}$ and $1{\cdot}26 \times 10^{-11}$ dynes. cm./radian, we have calculated all the frequencies. Their values, together with the observed frequencies (in brackets), are given below:

Class A_1, 3017, 1668, 874, 200 (3013, 1655, 877, 200); class A_2, 1000; class B_1, 3007, 1665, 111 (3009, 1642, —); class B_2, 1751, 588; class E_1, 2987, 1718, 1169, 826, 254 (2970, 1734, 1208, 802, —); class E_2, 3012, 1665, 1308, 929, 309, 199 (3003, 1639, 1209, 964, 370, 293); class E_3, 3009, 1640, 1555, 899, 123 (2954, 1608, 1442, 953, 254).

The agreement between observed and calculated values is satisfactory. We have also explained the remaining fifteen weak Raman and twenty-five infra-red frequencies as combinations of these fundamentals. Two points are, however, to be noted: first, the very strong infra-red frequency at 675 has to be explained as a combination, and secondly the 1655 line has to be interpreted as a hydrogen frequency. The latter, however, is not improbable in view of the assignment of 1520 as a C—H ⊥ oscillation in benzene by Lord and Andrews[7].

Taking the data of Kaufman and others, namely, C=C bond as 1·34 A., C—C bond as 1·54 A. and angle C—C—C as 125°, we have calculated the ring frequencies of the D_4 structure for various values of C—C, C=C valence and C—C—C deformation force constants. It is found that much better agreement with observed values is obtained for equal than for unequal values of C—C force constants. The polarized ring frequencies are calculated as 202, 870, 1670 for C—C and C=C force constants each equal to $6{\cdot}08 \times 10^5$ dynes and C—C—C deformation constant as $0{\cdot}36 \times 10^{-11}$ dynes. This shows that 1655 need not be regarded as a C=C frequency.

The Raman effect evidence, therefore, appears to be in favour of the resonating structure D_{4d}, although at this stage of our investigation we cannot rule out the D_4 structure.

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HARI NARAIN

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University of Allahabad.
Dec. 19.

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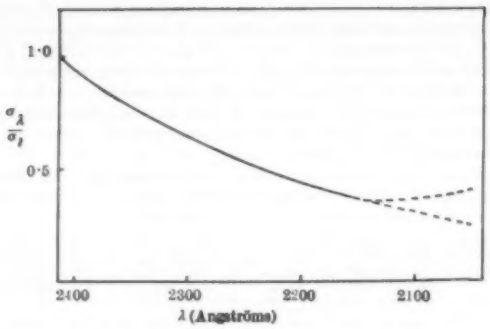
[7] Lord and Andrews, *J. Phys. Chem.*, **41**, 149 (1937).

Continuous Absorption of Light in Sodium Vapour

THE continuous absorption of light in sodium vapour, corresponding to the photo-ionization of the atom, has been measured with an apparatus generally similar to that used by Ditchburn, Tunstead and Yates[1] for potassium vapour. Under the conditions of the present experiment, the absorption due to molecules is small compared with that due to atoms at the series-limit (2411 A.). After correcting for the molecular absorption, the atomic absorption coefficient at the series-limit (σ_l) is found to be $1{\cdot}16 \times 10^{-19}$ cm.^{2}. This value is subject to an error of ± 10 per



cent, about half of which is due to uncertainty in the vapour pressure[3]. The variation of absorption coefficient with wave-length is shown in the accompanying graph, σ_λ/σ_l being plotted as a function of λ down to 2150 A. The accuracy of photometry decreases rapidly as the wave-length decreases beyond this point, and we can only state that the absorption lies between the two dotted curves. It appears probable that there is a minimum in the absorption curve at, or near, 2150 A.; but this cannot be stated with certainty.

The value of σ_l has been calculated by Trumpy[3], who obtains the value $3{\cdot}1 \times 10^{-19}$ cm.^{2}, and by Rudkjøbing[4], who obtains $1{\cdot}6 \times 10^{-19}$ cm.^{2}. Rudkjøbing's results indicate that the absorption at 2110 A. is a few per cent of σ_l, and that there may be a minimum near this wave-length. It is recognized that the approximations made in the calculation are such that high accuracy is not possible. The later calculation and the experimental result are therefore in reasonable agreement.

Note added in proof. The following values for σ_l have recently been obtained by W. J. Seaton (private communication): $1{\cdot}0 \times 10^{-19}$ using a dipole length formula and $0{\cdot}7 \times 10^{-19}$ using a dipole velocity formula. The variation with wave-length follows the general shape of the accompanying graph, coming to a minimum below the lowest wave-length covered by observations.

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Dec. 21.

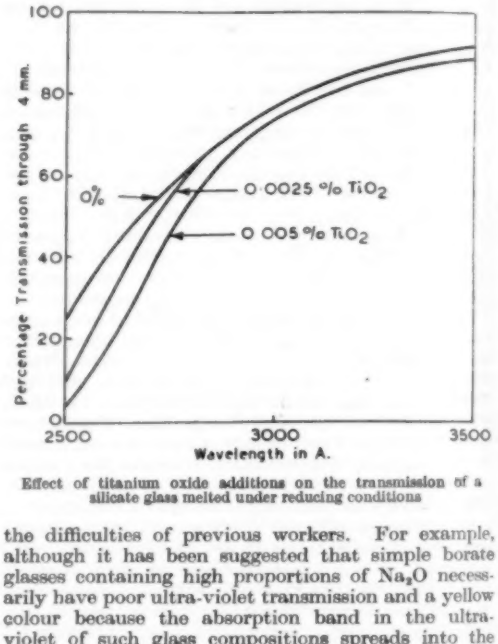
[1] *Proc. Roy. Soc.*, A, **181**, 386 (1943).
[2] Ditchburn and Gilmour, *Rev. Mod. Phys.*, **13**, 310 (1941).
[3] *Z. Phys.*, **71**, 720 (1931).
[4] *Pub. Kobenhavn Obs.*, **13**, 1 (1941).

Transmission of Bactericidal Radiation through Glass

In a previous communication[1], I made known the successful preparation of a soft-soda glass containing 0·01 per cent of iron oxide and with excellent transmission at 2537 A., even though previous workers had found zero transmission at this wave-length through soft-soda glasses with the same iron oxide content[2].

There was an obvious possibility that the composition of the soft-soda glass used by us was specific in the sense that the major constituents in some way promoted good ultra-violet transmission. This possible explanation of the discrepancy between our results and those of Starkie and Turner was very easily dismissed by melting under our conditions the soda–lime–silica glass used by them, and having the percentage composition SiO_2 75, CaO 10, Na_2O 15. The batch was melted in a gas-fired furnace in a platinum crucible and using a small addition of aluminium powder to ensure strong reducing conditions during melting; the glass had excellent ultra-violet transmission almost identical with that claimed for our own glass in the previous communication.

The failure of previous workers is probably to be attributed either to incomplete reduction to the ferrous condition of the small percentage of iron still remaining in the glass, or to the presence of other impurities also harmful to ultra-violet transmission. In connexion with the latter we have found that small amounts of sulphur, iodine or titanium are important. Thus, the presence of sulphur as an impurity in commercial barium carbonate markedly reduces the ultra-violet transmission of the soft-soda glasses when melted under the conditions referred to above, and, in fact, also promotes a yellowish colour. This effect of sulphur may explain some of



Effect of titanium oxide additions on the transmission of a silicate glass melted under reducing conditions

the difficulties of previous workers. For example, although it has been suggested that simple borate glasses containing high proportions of Na_2O necessarily have poor ultra-violet transmission and a yellow colour because the absorption band in the ultra-violet of such glass compositions spreads into the visible region, we have been able to make colourless sodium borate glasses containing 25 per cent Na_2O and with *c.* 75 per cent transmission at 2537 A. through a thickness of 3 mm.

Another harmful impurity in simple sodium borate melts is iodine. Thus, in a sodium borate glass containing 12·4 per cent Na_2O, the addition to the melt of 0·15 per cent iodine in the form of potassium iodide (not all of which would be retained in the finished glass) reduces the transmission at 2537 A. through a thickness of 4 mm. from 82 per cent to 35 per cent without producing any visible colour in the glass.

We have found that a most important impurity in silicate glasses melted under the reducing conditions

already described is titanium, which produces a marked effect on 2537 A. transmission when present in quantities even as low as 0·002 and 0·005 per cent, as shown by the results in the accompanying graph. It has not been realized before how harmful such small percentages of titanium can be. It is unfortunate that sands available in Great Britain contain titanium; there are American sands which are not only lower in iron content than British sands, but are also free from titanium. The melts referred to in the graph were made using an American sand containing no titanium when examined spectrographically. These facts are of importance in connexion with the possible manufacture in Great Britain of germicidal lamps.

It is hoped to publish our findings in detail in the near future. I am grateful to Mr. L. J. Davies, director of research, for permission to publish this letter.

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Jan. 4.

[1] Stanworth, J. E., *Nature*, **161**, 856 (1948). It should be noted that the letters *a* and *b* on the curves were accidentally interchanged in this earlier communication.

[2] See, for example, Starkie, D., and Turner, W. E. S., *J. Soc. Glass Tech.*, **12**, 324 (1928) and **15**, 365 (1931).

Application of the Reynolds Analogy to Combustion of Solid Fuels

The analogy which was pointed out by Osborne Reynolds between heat transfer and friction in the flow of fluids is well known. In its simplest form the relation is established by an argument of the following type. The frictional resistance is considered to be due to the transport of molecules to the surface, where their mass motion at the average velocity v is considered to be stopped. The heat transfer process is considered to be due to molecules which reach the surface coming into temperature equilibrium with the surface and then remixing with the rest of the fluid. The simplest equation from the Reynolds analogy is obtained by considering that the molecules which reach temperature equilibrium with the surface in the heat transfer process are those the mass motion of which is stopped at the surface in friction process so that the numbers can be equated. The resultant relationship is

$$f = \frac{2h}{\rho c_p v}, \qquad (1)$$

where f is the friction coefficient, h is the heat transfer coefficient, ρ is the fluid density and v is mass velocity, c_p being its specific heat, at constant pressure for a gas.

This equation gives very satisfactory agreement for gases for which the thermal diffusivity is nearly equal to the kinematic viscosity, as shown by McAdams[1]. The agreement is particularly satisfactory for air, and this fact prompted me to consider whether a similar analogy could be applied to combustion processes with solid fuels. The procedure is fairly obvious and may have been anticipated, although so far as I am aware it has not previously been pointed out. The argument proceeds as follows.

It is known that in the combustion of solid fuels under practical conditions, when a sufficiently high temperature is reached, the temperature of the surface can cease to be a controlling factor, and the rate at which oxygen can be transported to the surface becomes of governing importance. The extension of the Reynolds analogy applies to this case. Let us assume that the number of molecules transported to the surface is given in terms of either the friction coefficient or the heat transfer coefficient as in the Reynolds analogy, and then make the assumption that all the oxygen molecules in these molecules transported to the surface enter into reaction with the surface. If it is also assumed that the partial pressure of oxygen in the mass transported to the surface is the same as in the bulk of the fluid, we then have a simple equation for the mass reacting per unit time. With air as the fluid and carbon as the solid fuel, we obtain, if carbon dioxide is the product,

$$R = 0{\cdot}0875 \frac{h}{c_p}, \qquad (2)$$

where R is the reaction-rate expressed in weight of carbon per unit time per unit area of carbon surface.

Expressed in terms of friction coefficient, the similar relationship becomes

$$R = 0{\cdot}0875 \frac{\rho v f}{2}. \qquad (3)$$

Since the relationship in equation 1 does not hold exactly, the suggested extension to combustion processes may be used either in the form of equation 2 or in the form of equation 3, whichever is found to give closer agreement. That the orders of magnitude predicted by these equations are correct is immediately seen from the following calculation.

For air, h/c_p is of the order 10^{-3} gm. per sec. per sq. cm., giving a value for R of the order 10^{-4} gm. per sec. per sq. cm. This is the order of magnitude actually found in experiments, and also in certain practical circumstances when low-volatile fuel is burned on grates.

Equations 2 and 3 also account fairly well for the variation of combustion-rate with velocity of air flow, and with pressure drop, across fuel beds, because both h and f vary with velocity. The problem is complicated in practical cases with thick fuel beds, because of the secondary reaction where carbon dioxide reacts with carbon to form monoxide which is then burnt; but even there the method can be used to advantage. In developing the application, the partial pressure of oxygen at the surface can be allowed to vary, while still retaining the general concept.

In previous work on combustion-rates by Davis and Hottel[2], and by Parker and Hottel[2], the rate at which molecules were transported to the solid surfaces by diffusion was calculated. The agreement there is fairly satisfactory; but the formulæ required are exceedingly complicated, and as a result are unsuitable for use in many industrial research and design applications. To use equations for turbulent diffusion would be even more difficult. The use of the above development from the Reynolds analogy offers a very great simplification for both laminar and turbulent conditions, since the molecular transport can be calculated from heat transfer data observed on similar beds of inert particles, or even more easily from observations of the pressure drop and friction coefficient in identical beds of non-burning fuel.

The results are sufficiently accurate to correspond with practice, and therefore can provide qualitative

information on the trends of related variables; and this kind of information, provided the orders of magnitude are correct, is often the most useful in practice.

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Dec. 27.

[1] McAdams, W. H., "Heat Transfer", 171 (New York and London: McGraw-Hill, 2nd edit., 1942).
[2] Davis, H., and Hottel, H. C., *Indust. and Eng. Chem.*, **26**, 889 (1924).
[3] Parker, A. S., and Hottel, H. C., *Indust. and Eng. Chem.*, **28**, 1334 (1936).

Absolute Measurement of Microwave Power by Radiation Pressure

IN an earlier note[1], a method of measuring microwave power by radiation pressure was described. Some experimental results have now been obtained. The power to be measured was fed through an *uncompensated* wave-guide T-junction to a matched water-load forming part of a balanced calorimeter power-measuring apparatus of the type described by L. B. Turner[2]. This provides a reliable independent measurement of the power.

The third leg of the T-junction consists of a short length of circular guide, slot-excited from the main rectangular wave-guide, and containing a movable reflector so placed that no reflexion occurs at the junction. The force acting on the reflector is $\frac{P}{2c} \cdot \frac{\lambda}{\lambda_g} \cdot \frac{1}{m}$, in which m is a coupling constant[3] having the value unity for a compensated T-junction. This force is measured by a torsion suspension, angular movement being indicated by a lamp, mirror and scale, and the whole system being enclosed to shield it from draughts. The factor $1/m$ was evaluated both theoretically and experimentally by two independent methods, giving the values 5·84 and 5·80 respectively.

Initial experiments using an aluminium foil disk showed that the movement was very greatly overdamped by air friction, and that changing temperature gradients leading to circulating air currents within the cavity caused serious instability of the zero setting. The disk was therefore replaced by a system of three concentric wire rings, which provides almost perfect reflexion of the H_{01}-mode, but has much less air resistance, so eliminating these two troubles. The value of $1/m$ for the ring system is approximately 5·4.

The equipment in this form was used for measuring the mean power of a 9·1-cm. magnetron transmitter giving approximately 1-μsec. pulses at about 1,000 c./s. repetition frequency. It was found that on switching on the transmitter, the torsion arm oscillated, with a natural period of about 40 sec. and a decay factor of about 0·7 per period, about a mean deflexion which increased over a few minutes in a roughly exponential manner to a limiting value about 15 per cent greater than the initial mean value.

The initial mean deflexion, found by extrapolation, is due to radiation pressure, which acts instantaneously, and was found to correspond to a force of 0·014 dyne, which is equivalent to a power of 36 watts, as compared with 32 watts measured by the calorimeter.

The reason for the gradual increase in deflexion is not known with certainty; but it seems probable that a heating effect of some kind is responsible, perhaps similar to that encountered by Nichols and Hull[4]. It is hoped that work now in progress will elucidate this phenomenon and avoid the uncertainty of extrapolation.

An obvious remedy for some of the effects of heating is to evacuate the apparatus, and it might be possible in these circumstances to revert to the use of a disk. It is emphasized that the method is absolute in the sense that measurements of mass, length and time only are involved in calibrating the radiation pressure meter.

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Jan. 4.

[1] *Nature*, **163**, 403 (1949).
[2] *J. Inst. Elect. Eng.*, **93**, IIIA, No. 9, 1467 (1946).
[3] Watson, W. H., "Wave Guide Transmission and Antenna Systems", 115 (Oxford Univ. Press).
[4] *Proc. Amer. Acad. Arts and Sci.*, **550** (1902–3).

Observed Changes in the Angle between the Magnetic Axis and the Geometric Axis of a Bar Magnet

THE standard instrument for declination measurements at the Magnetic Observatory, Hermanus, is an Askania standard magnetometer-theodolite of the Schmidt type. The two declination magnets supplied with the instrument are in the form of solid steel cylinders of length 7 cm. and diameter 1·2 cm., fitted with plane mirrors at both ends. The telescope is of the usual 'auto-collimating' type. Provided the angle between the magnetic axis and the normal to a particular mirror surface remains constant, the difference between the 'magnet erect' and 'magnet inverted' readings ($E-I$) provides a ready control during observations.

Whereas, in the past, observed changes in these $E-I$ differences were ascribed to slight movements of the mirrors relative to the body of the magnet, it has now been established that in some cases the effect is wholly or partly due to changes in the angle between the magnetic and geometric axes of the magnet. A striking example occurred on November 29–30, 1949, when one of the declination magnets at Hermanus was accidentally touched with a small steel rod used for damping the oscillations of the magnets during observations. As a result, the $E-I$ differences for the two mirrors of this magnet changed by 8′ 48″, and 8′ 46″, respectively.

This phenomenon raises the question of the desirability or otherwise of placing two declination magnets side by side in the same box when not in use. While it may be assumed that, as in the case of the magnetic moment, the relative orientation of the magnetic and geometric axes of a bar magnet will change under the action of an applied field, our experience shows that the $E-I$ differences observed in declination measurements remain reasonably constant, provided the magnets do not suffer physical contact with other steel objects. However, the small day-to-day differences observed during the routine observations may very well be due to changes in the magnetic properties of the magnets. Provided no such changes occur during a set of observations with

a particular magnet, the mean of the 'erect' and 'inverted' readings will still determine the true direction of the magnetic meridian.

As I have failed to find any reference to this effect in the literature immediately available to me, and as other considerations (such as the design of declination magnets, the effect of reversing the polarity of a dip needle, etc.) are involved, I should be interested to learn whether this effect has been previously reported, or encountered, by other observers.

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Dec. 20.

Figure of Merit for Servo Motor

A SERVO motor must control the position of a mechanism (valve, rudder, gear, etc.) in accordance with varying requirements. The figure of merit of such a motor indicates how speedily the motor is able to respond to these requirements.

A careful search of the literature shows that previous authors give as the figure of merit the torque to inertia ratio of the motor; H. Harriss takes exception to this in a recent paper by indicating that this figure does not give correct results[1]. The torque to inertia ratio is evidently an exact measure of the motor acceleration without load. In cases in which a load is connected to the motor through a reduction gear, the motor acceleration is

$$\ddot{\varphi}_M = \frac{T}{I_e + \frac{1}{n^2} I_L}, \qquad (1)$$

where $\ddot{\varphi}_M$ is motor acceleration, T is motor torque, I_e is motor armature inertia, n is gear ratio, I_L is load inertia. Therefore, the torque to inertia is still the figure of merit for maximum motor acceleration in most practical cases where $I_L/n^2 << I_e$. It is easy to believe that this ratio also implies maximum load acceleration, unless it is remembered that we may choose the gear ratio. The optimum gear ratio distributes the kinetic energy equally between motor and load, as will be shown herewith.

$$\ddot{\varphi}_L = \frac{nT}{n^2 I_e + I_L}, \qquad (2)$$

where $\ddot{\varphi}_L$ is the load acceleration. $\ddot{\varphi}_L$ must be maximum or the reciprocal a minimum. Therefore,

$$\frac{\partial\left(\frac{1}{\ddot{\varphi}_L}\right)}{\partial n} = \frac{I_e}{T} - \frac{1}{n^2}\frac{I_L}{T} = 0,$$

from which follows $n = \sqrt{I_L/I_e}$.

This value of n inserted into equation (2) yields

$$\ddot{\varphi}_L = \left(\frac{1}{2\sqrt{I_L}}\right)\left(\frac{T}{\sqrt{I_e}}\right) \text{ (must be maximum).}$$

For any given load I_L the figure of merit is

$$T/\sqrt{I_e}.$$

As an example, let us compare two motors.

Motor No. 1	T = 2 oz.-in.	I_e = 0·03 oz.-in.^{2}
Motor No. 2	T = 8 oz.-in.	I_e = 0·3 oz.-in.^{2}

Formula for maximum load acceleration:

	Motor No. 1	Motor No. 2
$T/\sqrt{I_e}$ =	11·6	14½

Of the two motors, motor No. 2 is the better.

Using the formula T/I_e would indicate incorrectly that motor No. 1 is the better.

	Motor No. 1	Motor No. 2
T/I_e =	66⅔	26⅔

The new figure of merit is valid in all cases where friction and gear inertia can be neglected.

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Air Force Cambridge Research Laboratories,
Mass. Jan. 20.

[1] Amer. Inst. Elect. Eng., Tech. Paper No. 47-12 (November 1946).

Recurrence Times

CHANDRASEKHAR has remarked[1] that "the theory of density fluctuations as developed by Smoluchowski represents one of the most outstanding achievements in molecular physics", and has an important bearing on the interpretation of the Second Law of Thermodynamics. In view of the great value of Chandrasekhar's review of this and other stochastic problems in physics and astronomy, it seems desirable to correct some somewhat subtle errors of principle in his discussion of Smoluchowski's theory[2] of recurrence times, in case these have not previously been noticed.

For any stationary process (that is, in statistical equilibrium) with a finite number of possible states, we may define a 'lifetime' t and a 'recurrence time' θ for any specified state 'n'. Chandrasekhar's method of deriving the complete distribution for t (chapter 3) applies to any *Markoff process* (that is, a process the 'memory' of which does not extend beyond the previous instant); even then it is erroneous to discuss θ by applying a similar argument to the state 'not n'. The latter is a mixed state, and a process does not remain Markovian if some of its states are not distinguished. Consider, for example, a deterministic (and hence Markoff) system defined for discrete time and with only three states A, B and C, which follow each other cyclically. Given 'not A' at any time, the system must be in state B or C, and hence in C or A at the next instant; but given 'not A' on two consecutive occasions, the system at the next time must be in A, and thus has a memory, as regards the states A and 'not A', extending back before the previous instant. Actually, in the theory of density fluctuations, even the state n is a mixed state, referring to the number of particles in an element of volume which itself is a sum of smaller elements.

However, we may always obtain formulæ for the *average* values T and Θ of t and θ (suitably defined) by general arguments applicable to any stationary process for which the ergodic property holds, and essentially identical with Smoluchowski's original arguments. For example, in a long enough total time interval I, the time spent in any state n will be IP, where P is the probability of state n. Hence the number N of excursions from n to 'not n' is given by $NT=IP$. The total time available for such excursions before return to n is $I(1-P)$, whence $\Theta P=T(1-P)$. Smoluchowski's relation connecting T with the conditional probability of n, given n at the previous instant, also holds in general.

The definition of θ implied in the above argument is that of a true recurrence time, and is not the same as Chandrasekhar's time of recurrence to n from "*an arbitrary state which is not* n". We may, in fact, following Smoluchowski's own distinction between the two definitions, denote the times θ on the two definitions by θ_1 and θ_2 respectively. Similarly, we

let t_1 denote a complete lifetime in n, and t_2 the further lifetime from an arbitrary instant at which the state is n. It is stressed that only a relation for *mean* times has been deduced above, though it is remarkable that by a simple extension of Smoluchowski's arguments we may obtain at least one relation between the complete distributions of θ_1 and θ_2. This is

$$\Theta_1\Pi(z\,;\,\theta_2) = z(1 - \Pi(z\,;\,\theta_1))/(1 - z) \qquad (1)$$

for discrete time, or

$$\varphi\Theta_1 M(\varphi\,;\,\theta_2) = M(\varphi\,;\,\theta_1) - 1 \qquad (2)$$

for continuous time, where $\Pi(z)$ denotes a probability-generating function and $M(\varphi)$ a moment-generating function. Similar relations hold between t_1 and t_2. However, for *any Markoff process* the lifetime in state n does not depend on the history prior to the last instant, and t_1 and t_2 are in that case stochastically equivalent; the relation similar to (2) then leads to the familiar exponential distribution for t_1 or t_2. Conversely, from (2), except under conditions for which θ_1 and θ_2 (or t_1 and t_2) become stochastically equivalent, the exponential distribution *cannot* hold exactly.

It thus still remains to determine their distributions. One possible method available for Markoff processes is to modify the standard matrix representation of the transition probabilities; this matrix, operating on any vector representing the initial probability distribution of states, generates the succeeding distributions. If we modify the matrix by prohibiting transitions *from* state n, we eliminate from all 'not n' at any instant all 'paths' which *at any time previously* moved into state n, and hence we obtain any required information on times of recurrence (which are lifetimes in 'not n'). The distinction between θ_1 and θ_2 is obtained by appropriate choice of the initial vector, though from (1) or (2) only one distribution need be found.

(*Note added in proof, April* 27. A somewhat more general method based on renewal theory has recently been indicated by Feller[3]. However, as the definition of recurrence time is different, some adaptation is necessary, and the method above seems the more convenient in the present context; it readily gives, for example, the general distributional form, or the explicit condition for the exponential distribution.)

Further investigation of these distributions is being undertaken in collaboration with Mr. B. J. Prendiville.

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Dec. 19.

[1] Chandrasekhar, S., *Rev. Mod. Phys.*, **15**, 1 (1943).
[2] Smoluchowski, M. v., *Wien. Ber.*, **124**, 339 (1915).
[3] Feller, W., *Trans. Amer. Math. Soc.*, **67**, 98 (1949).

Role of Hydrogen Molecule Ions in Aqueous Solutions

The hydrogen molecule ion H_2^+ has played a important part in theoretical chemistry, and its existence in the gaseous phase is well established. Some of our recent experimental work on the reactions of hydrogen atoms in aqueous solutions seems to suggest that a somewhat similar ion exists also in the aqueous phase, where one may have the equilibrium:

$$H + H^+.aq \rightleftarrows H_2^+.aq. \qquad (1)$$

One might be inclined to assume that this equilibrium is shifted very far towards the dissociation side because of the large heat of hydration of the proton. This, however, will be counteracted to some extent by the bond energy (2·65 eV.) of the H_2^+ molecule and also if this ion possesses a heat of hydration of considerable magnitude (its hydrated form is then perhaps better represented by H_4O^+.aq. similar to H_3O^+.aq).

From the chemical point of view the interest lies in the fact that, whereas hydrogen atoms themselves are strong reducing agents (electron donors), a hydrogen atom combined with a hydrogen ion should act as electron acceptor and exhibit oxidizing properties according to:

$$H_2^+.aq + \text{electron (from donor)} \rightarrow H_2. \qquad (2)$$

leading to the formation of molecular hydrogen, as, for example, the simple electron transfer processes:

$$H_2^+ + I^- \rightarrow H_2 + I \qquad (2a)$$

$$H_2^+ + Fe^{3+} \rightarrow H_2 + Fe^{2+}. \qquad (2b)$$

We have studied a number of aqueous systems at different *p*H in the presence of hydrogen atoms which were produced either by ordinary (thermal) reactions or photochemically (for example, by the irradiation of certain simple ions in the near ultraviolet[1]) or by the action of ionizing radiations (X-rays, γ-rays, etc.) on water[2], and reactions of the type discussed above appear to be of significance, particularly at lower *p*H values.

A full account will be published elsewhere.

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Jan. 12.

[1] Cf. Weiss, J., *Trans. Farad. Soc.*, **37**, 463 (1941).
[2] Cf. Weiss, J., *Nature*, **153**, 748 (1944).

Steric Inhibition of Resonance in some Organic Copper Compounds

Copper diphenylacetate is insoluble in benzene and practically so in ethyl alcohol, but is readily soluble in a mixture of these solvents due to the formation of a compound with two molecules of alcohol[1]. Similar behaviour is now found in the copper salts listed in Table 1. Copper *o*-iodobenzoate, for example, crystallizes with one molecule of alcohol.

Table 1

Solubilities (gm./100 ml.) of copper salts in benzene containing 10 per cent ethyl alcohol at 25° C.

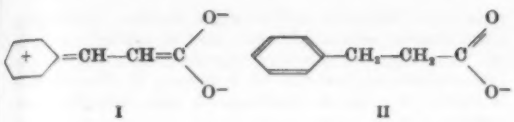
Phenylacetate	2·73	Butyrate	18·3
Diphenylacetate	7·65	*o*-Toluate	5·58
Triphenylacetate	r.s.	*o*-Chlorobenzoate	r.s.
Hydrocinnamate	r.s.	*o*-Bromobenzoate	r.s.
Propionate	7·64	*o*-Iodobenzoate	r.s.

Table 2

Benzoate	0·13	*p*-Toluate	0·01
Cinnamate	0·44	*p*-Chlorobenzoate	i.
Crotonate	0·31	*p*-Iodobenzoate	i.

r.s. = readily soluble; i. = insoluble.

Table 2 lists copper salts found to be insoluble or nearly so in the mixed solvents. It is noted that in these the carboxyl group is conjugated with a double bond in the αβ-position. This will facilitate stabilization of the ion by resonance as in (I). Such ionic



I II

forms will build into a stable crystal lattice and be insoluble in benzene. Where coupling of resonance between the ring and the carboxyl group is impossible as in (II), the associated giant molecule of the copper salt is indeed insoluble in benzene; but added alcohol combines and causes disaggregation to small molecules soluble in benzene.

The solubility of the copper salts of *o*-substituted benzoic acids, at first sight surprising, can be explained also by lack of resonance coupling between the ring and the carboxyl group. Here the large ortho-group forces the oxygen atoms out of the plane of the ring, so hindering resonance. The ionic forms are consequently not stabilized sufficiently to give a stable insoluble crystal lattice, and the molecule is again disaggregated by alcohol to a soluble form. Such sterically hindered resonance has been observed to affect various physical properties in *o*-substituted compounds[1–8].

It is further suggested that ortho effects in *o*-substituted benzoic acids, not readily accounted for by chelation, are due to non-co-planarity of the carboxyl group and the ring[9–11] even in the absence of the large copper atom present in these salts studied.

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Dec. 16.

[1] Crawford, M., *Nature*, **160**, 19 (1947).
[2] Brown, W. G., and Fried, S., *J. Amer. Chem. Soc.*, **65**, 1841 (1943).
[3] Kadesch, R. G., *J. Amer. Chem. Soc.*, **66**, 1207 (1944).
[4] Thomson, G., *J. Chem. Soc.*, 404 and 408 (1944); 1113 (1946).
[5] Remington, W. R., *J. Amer. Chem. Soc.*, **67**, 1838 (1945).
[6] Brown, W. G., and Reagan, Miss H., *J. Amer. Chem. Soc.*, **69**, 1032 (1947).
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Fluorescent Development of Adrenaline in Paper Chromatograms

JAMES[1] described the use of potassium ferricyanide for the development of adrenaline in paper chromatograms. The purpose of this letter is to state that such development may also be achieved by fluorescence. In alkaline solution adrenaline produces an apple-green fluorescence when exposed to ultra-violet light[2]. The fluorescent substance is said to be leucoadrenochrome[3].

The solvent used was liquefied phenol. After this had been removed by drying, the strip was exposed to ultra-violet light in the vapour of concentrated ammonia. The position of the adrenaline in the chromatogram was indicated by the appearance of the typical fluorescence within one minute. Complete removal of the phenol was not necessary.

Using this method, 10 μgm. of adrenaline can be identified in 18-hr. runs. Smaller quantities have been demonstrated in shorter runs.

Development with ammonia vapour does not cause smudging such as may occur when a spray is used. Development with ferricyanide is still possible immediately after the ultra-violet demonstration.

No experiments were carried out with *nor*adrenaline or other adrenaline-like compounds.

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Staphylococcal Coagulase: the Nature of Plasma Activator in the Clotting Process

IN 1944, Smith and Hale[1] showed that the clotting of fibrinogen by coagulase depends on an activator present in the plasma of certain animals, notably humans and rabbits. Since a fibrinogen–prothrombin complex prepared from human plasma by Mellanby's[2] acid precipitation method, which could be clotted by tissue kinase, could not be clotted by coagulase, they concluded that the activator was not prothrombin. Later, Tager[3] and Tager and Hales[4] noted certain resemblances between the physical properties of activator and prothrombin, both being precipitated with the globulin fractions in the region of *p*H 5·0 and by 50 per cent saturation with ammonium sulphate. In ethanol-precipitated fractions of plasma prepared by the method of Cohn[5], activator was present in fractions III-1, III-2 and IV-1, whereas prothrombin is believed to occur only in III-2. Both prothrombin and activator were absorbed from plasma by barium sulphate; but considerable dissociation followed repeated Seitz filtration of human plasma, prothrombin activity being lost without comparable loss of activator. Studies by Kaplan and Spink[6] disagreed with these findings, inasmuch as they found activator to be associated with the albumin fraction in ammonium sulphate fractionation, and to lie mainly in IV-4 of the ethanol precipitate.

In the course of a study of the properties of coagulase, our attention was first directed to a possible relationship between activator and one of the blood-clotting factors by the observation that the greater part of the activator present in rabbit plasma disappeared rapidly during blood coagulation. A detailed study of the clotting mechanism using rabbit and human plasmas showed a parallel loss of activator and prothrombin on incubation with thromboplastin (acetone-dried brain extract) in the presence of an optimal concentration of calcium ions, there being little or no loss of either in the absence of free calcium. An examination of the plasmas of rabbits and humans receiving dicumarol showed good correlation between prothrombin—and activator—levels, although it is recognized that the method used for prothrombin estimation (Quick's[7] one-stage technique) is only semi-quantitative. Separation of prothrombin from factor V by absorption and elution of the former in freshly prepared aluminium hydroxide showed that activator was associated with prothrombin and not with the factor V residue.

In confirmation of Tager's work, activator as well as prothrombin was precipitated with the globulins by ammonium sulphate in rabbit and human plasma;

but there was poor agreement between the activator and apparent prothrombin content of different plasma fractions up to 40 per cent saturation with sulphate, at which level almost all of both constituents had been removed. This was in part due to removal of coagulase inhibitor which is present to some degree in all sera, and which is precipitated at the lower concentrations of ammonium sulphate. Likewise four Seitz filtrations of rabbit and human plasma removed prothrombin activity completely with only partial loss of activator. Quantitative comparisons after each filtration were again made difficult by the early removal of coagulase inhibitor, especially in the case of human plasma. This loss of prothrombin on Seitz filtration may be more apparent than real, since Tager and Hales found it to be partly restored by acid precipitation and resuspension. Like prothrombin, activator is removed from plasma by barium sulphate and is destroyed by crystalline trypsin and pepsin.

If prothrombin participates in coagulase clotting, it does so in a manner essentially different from that found in normal coagulation. Mixtures of coagulase and plasma show no loss in prothrombin content on clotting, as would be the case had there been conversion of prothrombin to thrombin. Likewise, mixtures of prothrombin, factor V and coagulase incubated at 37° C. show no progressive increase in the amount of coagulant present as occurs in the usual two-stage prothrombin conversion test. Coagulase itself disappears when clotting has taken place, and is presumably attached to the fibrin clot. Thus while coagulase activator is closely associated with prothrombin both in occurrence and physical properties, we cannot as yet conclude that they are identical, and the studies are being continued. The present results will be published in full elsewhere. We should like to thank Drs. R. G. Macfarlane, Rosemary Biggs and R. A. Kekwick for helpful advice and material.

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Production of Coombs's Serum

The production of a high-titre anti-human globulin serum is a matter of importance to a laboratory engaged in the detection of incomplete antibodies. Our experience with the usual methods of production has been disappointing; but we have obtained good results with the following course of injections.

Adult male and female rabbits of a mixed stock have been used. They are fed on bran mash and greenstuff. (1) Intramuscular injections of 5 ml. alum-precipitated globulin into each hind leg. The globulin is prepared by the method of Proom[1] as described by Mollison, Mourant and Race[2]. Interval of two weeks. (2) The intramuscular injections are repeated. Interval of four–eight weeks. (3) Intraperitoneal injection of 1 ml. sterile human serum. (4) Next day, intravenous injection of 1 ml. sterile human serum. Interval of 5 days. (5) The rabbit is bled, the serum inactivated and absorbed as usual.

This course of injections has now been carried through on six rabbits. No trouble from anaphylaxis has been encountered. Every rabbit has produced a reagent capable, when diluted 1 : 10,000, of agglutinating fully sensitized red cells. The strongest serum was reported by another laboratory as active when diluted about 1 : 256,000.

The sera are normally used diluted 1 : 50–1 : 100, since they are most active at this dilution. If sera of this potency are used at a greater concentration, their activity is less. It is hoped to give a fuller account of this finding elsewhere.

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Effect of Anions on the Respiration of Kidney Slices from Adult Rats

Slices of adult rat kidney have been found not to take up oxygen at a constant rate in media which are suitable for other tissues[1,2]. They were found to do so, however, in a medium containing physiological concentrations of sodium, potassium, calcium, magnesium, phosphate and chloride ions and glucose[2]. Because its ionic requirements are so critical, this was considered a suitable tissue to use in an investigation of the relative importance of the cations and anions in balanced media. Slices of adult rat kidney were therefore set up in Barcroft manometers under oxygen at 38·5° C. in media having the cation concentrations which had been found to support their steady respiration, but in which chloride was replaced by other anions.

Thus, an isotonic solution corresponding to the "Medium *A*.2" (ref. 3) was prepared by mixing 0·154 *M* sodium nitrate, 232 ml., 0·154 *M* potassium nitrate, 8 ml., 0·11 *M* calcium nitrate, 3 ml., and 0·154 *M* magnesium sulphate, 2 ml., and adding 24 ml. of phosphate buffer prepared by bringing *M*/15 disodium hydrogen phosphate to *p*H 7·4 with *N*/10 nitric acid. Glucose was added to give a concentration of 100 mgm./100 ml. In seven experiments using this medium, the oxygen uptake during the first hour was 4·2 (S.D. 0·4) µl. oxygen/hr./mgm. initial moist weight. Four control experiments set up at the same time in the chloride-containing medium *A*.2 gave a rate of 4·2 (S.D. 0·2) µl. oxygen/hr./mgm.

Another medium was prepared in which the sodium, potassium and magnesium ions were introduced as sulphates, and the calcium ion as nitrate. This medium had the same concentrations of all cations as *A*.2, and was therefore hypotonic on account of the divalence of the sulphate anion, its total osmolar concentration being about three-quarters that of the chloride and nitrate media. Two sets of slices in this medium gave respiratory rates of 4·7 and 4·8 µl.

oxygen/hr./mgm., while two sets of slices from the same rat in the chloride medium each gave a rate of 4·7 µl. oxygen/hr./mgm.

There was a tendency for the oxygen uptake to fall slightly during the second hour in the chloride-free media, though the slices appeared normal to the naked eye after two hours, and determination of their water content showed that no significant swelling had occurred.

These preliminary observations suggest that the normal respiration of adult rat kidney slices depends upon the proper concentrations of the various cations in the medium, and that the anions which accompany them are relatively unimportant.

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Intravascular Electronic Manometer

WE have recently developed in these laboratories a recording manometer which has been employed with success in the measurement of intra-arterial and intra-cardiac pressure-cycles in man.

A cardiac catheter or needle in the appropriate vessel is connected by a lead pipe to a pressure head fitted with a bronze diaphragm of diameter 15 mm., and thickness 0·22 mm., the whole being filled with sterile normal saline. The responses of the diaphragm to fluid pressure are measured with a mechano-electronic transducing valve, type *RCA* 5734. This valve translates mechanical deviation of a stylus into an electrical potential, and has the important characteristic of responding to zero frequency, thus making possible the recording of static as well as dynamic pressures. In our instrument a mechanical device brings the stylus of the valve into gentle and measurable contact pressure with the diaphragm, and the potential produced by subsequent pressure variations is measured with an oscilloscope and recorded photographically.

The sensitivity of the instrument is such that simple amplification only is required, and at the greatest sensitivity used, a full-scale deflexion of the oscilloscope is obtained with a pressure of 10 cm. of saline. By varying the degree of amplification any desired sensitivity may be chosen, making it possible to select any portion of the pressure curve and to magnify it for detailed examination.

Since the movement of the diaphragm is small, flow effects are virtually eliminated, and the instrument has a linear response to pressure changes.

A paper describing this manometer and its application to intra-cardiac pressure recording is in course of preparation.

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"Soviet Genetics: the Real Issue"

I DO not propose touching upon points where Dr. Julian Huxley and I must agree to differ in his recent *Nature* articles[1], so the relation of science and politics, and the 1948 Moscow discussion on genetics, will be ignored. I must, however, lay stress upon a point where Huxley is principally wrong.

Soviet science is mainly, though not completely, utilitarian. New biological theory based on experimentally proved facts opens up new perspectives for the development of new ideas and new aspects for practical application, especially in agriculture. These applications, though as yet not fully exhausted, have resulted in a remarkable rise of production within the Soviet Union.

The theoretical basis of the new Soviet science lies in the recognition of different developmental stadiums in the life of living organisms. Genetic potentialities of a living being in the different stadiums seem to differ markedly. This opens up the possibility of influencing organisms in certain different directions.

Environmental influences cause a disturbance of the hereditary characters—giving not strictly Lamarckian effects, but a state of 'shattered' inheritance, which may lead to stabilization of new features.

All this leads to a considerable smoothing down of the sharp line drawn between pheno- and genotypical changes.

My own experiments show that 'dauermodifications' or 'phenocopies' in *Drosophila* may, through selection, be changed into true mutations.

All these facts, as well as the results of vegetative grafting, must convince us that the fundamentals of classical genetics require a profound revision, especially in their Weissmannian aspects.

This calls for an exhaustive and impartial scrutiny of facts. Unfortunately, one cannot claim that this is the case in Dr. Huxley's article.

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No one disputes the recent rise in agricultural production in the Soviet Union. Prof. E. Ashby, in a recent broadcast (*Listener*, March 30, 1950), has directed attention to it, and to the fact that productivity "has even approached the yields reported for similar climates in Western countries". While, however, Dr. Marchlewski asserts that this is due to the application of "new biological theory" (presumably including new genetic theory), Ashby concludes that it was the result of the application of common sense and some very elementary practical measures, none of them involving genetics, to an extremely backward condition of agricultural practice. And he stresses that, to achieve this result, Lysenko was indispensable.

Dr. Marchlewski then makes certain genetical assertions of a revolutionary nature: (1) that different developmental stages (phases) of an organism differ in their genetic potentialities; (2) that there is such a condition as 'shattered' inheritance, which permits of something like Lamarckian effects; (3) that phenocopies can be "changed into true mutations" by selection. None of these points has yet been satisfactorily established in principle, and such empirical data relevant to them as are available

can be more satisfactorily interpreted on orthodox genetic and selection theory; furthermore, they appear incompatible with the much more firmly established facts and principles of neo-Mendelism.

The results claimed by Lysenko and his followers for vegetative grafting have so far proved completely unrepeatable (see report by L. Sachs in *Nature*, **164**, 1009; 1949), so that the presumption for the moment is that the empirical facts obtained in the Russian experiments were due to faulty technique or at any rate to causes other than the grafting itself.

Finally, I must strongly repudiate Dr. Marchlewski's accusation of bias. Throughout I have endeavoured to preserve the same scientific impartiality towards the claims of Michurin genetics as I would exercise towards any other new scientific claims; and, while it was difficult to be exhaustive in the space of an article, I have covered the field more widely in my book, "Soviet Genetics" (Chatto and Windus, 1949).

Dr. Marchlewski, perhaps wisely, merely "agrees to differ" from me over the question whether or not it is right for a non-scientific body to pronounce on the truth or falsity of rival scientific theories, and to enforce their acceptance or rejection. I am, however, glad to take this opportunity of re-asserting that this, as I indicated in my original article, is "the real issue", and much more important than the truth or falsity of particular claims to new results, Dr. Marchlewski's or Lysenko's or anyone else's.

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Extension and Radial Growth in Trees

IT has already been reported[1] that in the broad-leaved trees of India the extension growth or height growth usually precedes the radial growth, and that the gap between these two activities varies from two weeks to three months, irrespective of the fact whether the trees produce ring-porous or diffuse-porous woods. Since 1947, further investigation has been carried out at Dehrâ Dun on *Michelia champaca* Linn. and *Melia azedarach* Linn. with the view of finding any relation that might exist between initiation and propagation of the extension growth and of the radial growth.

In *Michelia*, an evergreen and diffuse-porous tree, the terminal buds of the previous year show swelling by the middle of February (Fig. 1,*a*) and the shoot extension is noticed in the third week of March. The height growth continues until the end of May (Fig. 1,*b*) and then stops. By that time the leaves on the new shoots are fully expanded; but there is no cambial growth either in the last year's shoot or in that of the current year. At about the end of June (June 29, 1949) the cambial activity, that is, radial growth, is initiated at the terminus of the last year's shoot, and progresses basipetally down the tree and acropetally towards the tip of the current year's shoot (Fig. 1,*c*; Fig. 2, *X*,*Y*,*W*,*Z*).

The extension growth starts for the second time about the middle of July (July 12, 1949), when the cambial activity in the current year's shoot does not usually finish. From then on, both the extension and radial growth continue simultaneously until the end of October or beginning of November (Fig. 1,*d*).

In the case of *Melia*, a deciduous and semi-ring-porous tree, signs of bud-swelling are noticed in early February, and the extension growth actually takes place in the second week of March. The height growth stops in the first week of May. During the period of first height growth there is no cambial activity. The radial growth starts some time in the last week of May at the terminus of the previous year's shoot and progresses basipetally down the tree and acropetally towards the tip of the current year's shoot. The height growth is once again noticed in the last week of June, and, from then on, the height and radial growth continue until the end of October. Thereafter, no change in growth has been noticed up to the end of the year.

These observations differ considerably from those reported from Europe and North America. In trees of those countries, both the extension and the radial growth start simultaneously[2,3,4]. The cambial growth is reported to start at the base of the opening bud and to spread basipetally in the last year's shoot and then down the main stem. Propagation of growth activities are, however, somewhat slower in the diffuse-porous trees than in the ring-porous trees. A point to note here is that for temperate countries no data appear to be available as to the direction in which the cambial activity progresses in the current year's shoot. We shall be interested to receive information on this point.

Most of the research work on natural and synthetic auxins has been confined to visual observations. Confirmation of visual observation by anatomical study has been rather limited[5,6]. As a result, no clear picture is now available, depicting the anatomical details of extension growth and of radial growth in normal trees in relation to auxins. According to Snow[6], the growth-hormone responsible for extension growth normally activates the cambial growth. Avery and his co-workers[5] have also reported that the centres of hormones are the terminal buds from where cambial activities begin. Söding[7,8] has demonstrated the presence of auxin in woody stems and has advanced the theory that auxin produced by the swollen buds remains in sufficient quantity in the cambium of the twigs to induce growth activity and to produce further auxin. However, in the light of our results, there would appear to be two distinct auxins involved in the growth activities of a tree—one auxin for the extension growth and the other

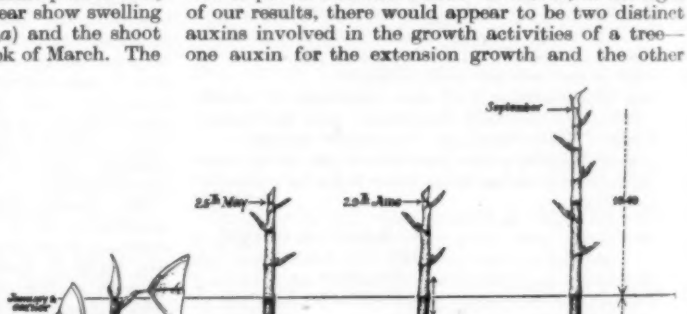

Fig. 1. *Michelia champaca* Linn.

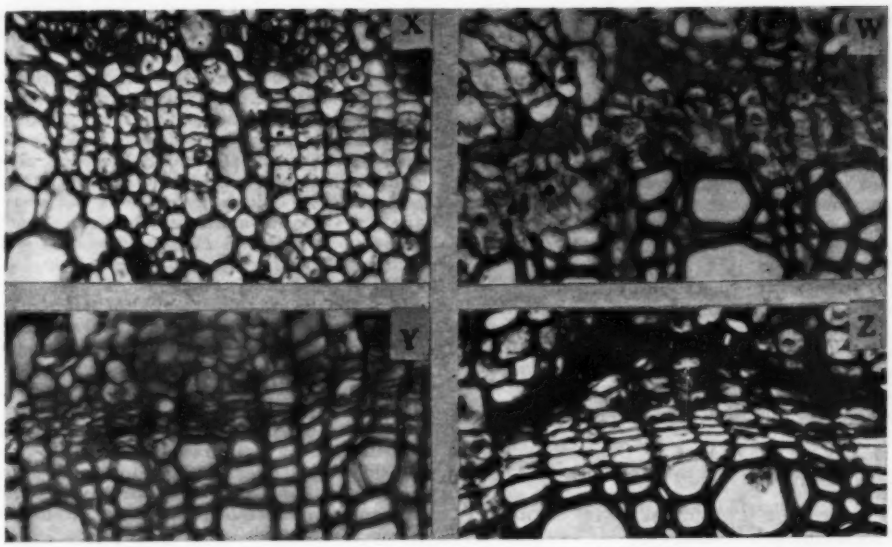

Fig. 2. *Michelia champaca* Linn. (all are cross-sections of Fig. 1, *c.* × 300): *X*, current year's shoot towards the tip, showing no radial growth; *Y*, the same shoot at the base, showing initiation of radial growth; *W*, shoot of the previous year at the tip, showing renewal of radial growth; *Z*, the same shoot lower down, showing no radial growth

for radial growth. Furthermore, these auxins seem capable of acting either singly or jointly, suggesting a possibility of their inter-conversion. Although the various stages in the chemical reactions of these growth substances are not now clearly understood, there are indications that they may not be so very different from what Fildes[9] and others have recently reported about the growth in bacteria.

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A Comparative Study of Bacteria Causing Mustiness in Eggs

Specific bacteria producing a penetrating musty odour in eggs have been isolated by Turner[1] (*Achromobacter perolens*), Spanswick[2] (*Achromobacter* sp.), Levine and Anderson[3] (*Pseudomonas graveolens*, *Pseudomonas mucidolens* and *P. muc.* var. *tarda*) and Grabiec, Szybalski and Tuszyński[4] (*Pseudomonas perolens* var. *Gdansk*). Jensen[5] obtained *P. graveolens* and *A. perolens* from musty meat. Olson and Hammer[6] claim that *P. graveolens* and *P. mucidolens* produce a potato odour in milk.

It was the purpose of the present work to compare all obtainable strains of 'musty' bacteria, that is, *A. perolens*[3] (No. 4430), *P. graveolens*[3] (No. 4683), *P. mucidolens*[3] (No. 4685) from the American Type Culture Collection, Washington, and *P. perolens* var. *Gdansk*[4].

At present only *P. perolens* var. *Gdansk*, *A. perolens* and *P. graveolens* were found to produce musty odours; the first species produces a very strong, the second a strong, and the third a very weak odour. *P. mucidolens* has apparently lost this property.

All strains have rod-shaped cells, and are Gram-negative, sporeless, and motile by means of a single polar flagellum. They ferment pentoses and reducing hexoses without production of gas but are unable to ferment any other sugars or alcohols (exception, see table). They are able to utilize lactate, citrate and ureate as single carbon sources, but not tartrate and oxalate. They have an optimum temperature range of 24–28° C., grow fairly well at 4° C., and are unable to grow at 37° C. Their thermal-death times at 60° C. are less than 5 min., and at 40° C. less than 3 days. All are aerobic and form circular, effuse iridescent, smooth colonies of butyrous consistency. In peptone water growth is abundant with a thin pellicle and a

	A. perolens	*P. graveolens*	*P. mucidolens*	*P. perolens* var. *Gdansk.*
Dimensions of cells (microns)	0·4 × 0·6–1	0·4 × 1–4	0·4 × 2–20	0·4 × 1–2
Gelatine liquefaction	−	−	+	+++
Litmus milk: slight acidification	−	+	−	−
peptonization	−	−	+	+++
reduction	±	±	+	+
Hydrolysis of fats	−	−	+++	+++
Nitrites (and gas) production	−	−	+	+++
H_2S-production	+	++	−	−
Arabinose fermentation	+	+	−	+
Rhamnose fermentation	±	±	−	±
Maltose fermentation	−	+	−	−

rich deposit. Potato medium yields a pale reddish-brown, glistening, vigorous growth of butyrous consistency. They produce catalase and ammonia, show positive methyl red test and do not produce indol and acetylmethylcarbinol. They are not able to grow in the presence of more than 0·05 per cent phenol.

Only *P. perolens* var. *Gdansk*, *A. perolens* and *P. graveolens* produce a green, fluorescent, water-soluble pigment, cause green egg-white, grow fairly well at 0° C., and use acetates as a single source of carbon.

Some other properties are summarized in the accompanying table.

It is proposed to place *Achromobacter perolens* (strain No. 4430) in the genus *Pseudomonas*[7] with the name *Pseudomonas perolens* because of a single polar flagellum as reported by Turner[1] and myself[4] and because of the production of a greenish, fluorescent, water-soluble pigment.

It is hoped to publish a fuller account of this work elsewhere.

My thanks are due to Prof. H. Dam for the hospitality of his laboratory and to Dr. E. Olsen for helpful discussions.

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Temperature Requirements of *Penilia avirostris* Dana in the Hauraki Gulf, New Zealand

In a pilot survey, still in progress, of the plankton of the Hauraki Gulf, New Zealand, *Penilia avirostris* Dana was found from December 1948 until April 1949. In December, *Penilia* was found in small quantities at some stations only; except in one case, the numbers were less than 1 per c.m., and *Penilia* was only recorded from stations of surface temperature 17·8° C. or above. In January, when apparently extensive multiplication occurred, as many as 1,250 per c.m. were recorded, more usual records being 300–400 per c.m. The modal recorded surface temperature for January was 20° C. In February, with a modal recorded surface temperature of 20·4° C., the numbers of *Penilia* declined to 50–150 per c.m., and continued to decline until April, when a sample with less than 1 per c.m. was recorded at a temperature of 18·7° C. This decline in numbers was reflected in other species. In both January and February, *Penilia* was frequently the dominant member of the plankton, sometimes forming more than 50 per cent of the planktonic animal population of coastal waters of depth 10–20 fathoms.

Although observations are so far available for one year only, the earlier record of Krämer[1] that *Penilia* is a normal member of the summer plankton of the Hauraki Gulf is apparently confirmed. Occurrence of *Penilia* in the Gulf is significant because observations made by the Marine Department, New Zealand[2], over a period of years show that the Hauraki Gulf lies just south of the 17° C. mean annual isotherm. Temperature observations in the coastal waters of the Gulf for the year December 1948 to November 1949, taken during the plankton survey, give a mean annual value of 17° C. It is clear, therefore, that as suggested by Calman[3], *Penilia* is not restricted by the 18° C. mean annual isotherm in New Zealand. It is not known if *Penilia* extends south of the Hauraki Gulf.

From these observations, the temperature requirements for the establishment of *Penilia* in a district appear to be: (1) the mean annual surface temperature may be as low as 17° C.; (2) a summer period is necessary in which the water remains at between 18° C. and 20° C. long enough for reproduction to occur.

Since neither of these conditions is found in the North Sea, it appears unlikely that *Penilia* will establish itself there, although it has been recorded[4], unless the North Sea offers unknown advantageous environmental conditions which will offset the lower temperatures.

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Occurrence of a Filter-feeding Mechanism in the Polychæte *Nereis diversicolor*

The Nereids as a group are regarded as typical examples of the predaceous errant polychætes[1]. *Nereis diversicolor* O. F. Müller (Annelida: Polychæta) has been recorded as feeding on small crabs, mussels, pieces of algæ, and also on detritus, and stress has always been laid on the role of the powerful jaws in seizing prey and dragging it down into the burrow[2]. The discovery of a filter-feeding mechanism is therefore of interest.

Evidence points to the conclusion that the filter-feeding funnel is made from long fine threads secreted by the parapodial glands[3,4] (*Spinndrüsen*[3]), and that these threads are moulded into the shape of a funnel by the parapodial setæ.

So far it has not been possible to determine the conditions under which *Nereis diversicolor* will use the filter-feeding mechanism; but the phenomenon has been observed many times in a number of different animals collected from two places. There can be no doubt that this mechanism is used, although its importance as a method of gathering food in the everyday life of the animal remains to be determined.

Worms were observed in pieces of glass tubing of suitable length and diameter immersed in a dish of sea water. When a suspension of carmine in sea water was added, the worm sometimes moved to one end of the tube and expanded laterally the anterior end of its body (from about the sixth to about the thirteenth parapodia-bearing segments), thus fixing the secretion of the parapodial glands to the wall of the tube. When the anterior end of the body relaxed

again, threads were seen to pass from the tips of the parapodia to the wall of the tube. The animal then retreated slowly down the tube; the anterior end moved repeatedly from side to side through a semicircle; and the first four to fourteen pairs of parapodia moved in circles, each slightly out of phase with its immediate neighbours, while the setæ were alternately protruded and withdrawn. These movements sufficed to make a feeding funnel from the threads secreted by the parapodial glands. The funnel when completed was attached in front to the wall of the tube and behind was almost completely blocked by the animal's peristomium. Meanwhile, most of the more distal segments of the animal undulated violently, drawing water laden with suspended particles past the animal antero-posteriorly. These undulating movements continued when the funnel was complete, and particles were thus automatically sieved out of the water current. The worm then moved forward and swallowed the feeding funnel with its entrapped particles in several gulps.

The numbers of parapodia involved in the various stages of the filter-feeding process and the time taken for each step vary considerably in one worm and from worm to worm. The following figures are summaries of observations from five worms: in a tube of 3 mm. internal diameter, worms 5–7 cm. in length made feeding funnels 0·5–2·5 cm. long; at 16·5–20° C. a funnel was made in 30–140 sec., water was pumped through the tube for 30–257 sec. after the funnel was completed, and a feeding funnel with its entrapped particles was swallowed in 6–16 sec.

The cycles of feeding may follow rapidly one after the other with a few short breaks, or they may occur with intervals of several minutes, or irregularly, for a total period of about two hours.

Feeding mechanisms similar in principle to this in *Nereis diversicolor* have been described for *Chætopterus variopedatus*[5], *Urechis caupo*[6] and some *Chironomus* larvæ[7].

These investigations form part of a study of the feeding habits of *Nereis*.

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University of Durham.

Dec. 20.

[1] MacGinitie, G. E., and MacGinitie, N., "Natural History of Marine Animals" (New York and London, 1949).
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Use of Glass Springs for Adsorption Measurements

THE use of spiral quartz springs in the McBain and Bakr[1] balance for adsorption measurements is well known. In stability and chemical inertness they are satisfactory; but they have disadvantages in high initial cost and fragility. Spirals of 'Pyrex' glass are cheaper, easier to fabricate, and more robust, but do not seem to have been much used, probably on account of lack of dimensional stability.

We have found that the stability of 'Pyrex' springs is considerably improved if they are annealed under load at a high temperature before use. Annealing at 200° C. for 8 hr. has been found to give a dimensionally stable spring suitable for use up to at least 100° C., and giving a linear load-extension relationship. The following results show the effect of the annealing treatment:

	Length of spring (cm.)
Originally, at 25° C.	16·209
After heating at 200° C. for 8 hr., then cooling to 25° C.	16·239
After further heating at 100° C. for 3 hr., and cooling to 25° C.	16·236

The dimensions of the spring were: fibre diameter, 0·008 in.; coil diameter, 0·75 in.; turns per inch, 12; sensitivity, 17·45 cm. per gm. The load in each case was 0·4 gm.

Springs annealed in this way have been in constant use at temperatures up to 100° C. and occasionally even up to 150° C. for periods of six months, and have shown no change in sensitivity over this period. They have been exposed to vapours of organic solvents, water and dry hydrochloric acid gas.

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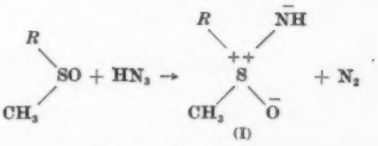
Dec. 17.

[1] McBain and Bakr, *J. Amer. Chem. Soc.*, **48**, 690 (1926).

Action of Nitrogen Trichloride on Proteins: a Synthesis of the Toxic Factor from Methionine

DIMETHYLSULPHOXIDE is converted by means of the Schmidt reaction[1] to a product identified as (I, $R = -CH_3$) by comparison with a specimen prepared by an alternative method[2].



Under comparable conditions DL-methionine sulphoxide gives, in moderate yield, a crystalline product toxic to rabbits at the same order of dosage as the toxic factor isolated from 'agenized' zein or wheat gluten, and shown by means of two-dimensional paper chromatography to be identical with this factor. The latter has previously been shown[3] to be an amino-acid, $C_5H_{12}N_2O_3S$, derived from methionine, and on the basis of the present synthesis has therefore the structure (I, $R = -CH_2CH_2CH(NH_2)COOH$).

In further work we shall endeavour to compare the toxicities of the several stereo-isomers of this compound. Some preliminary observations on this point have already been described[3].

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[1] "Organic Reactions", **3**, 307 (New York: John Wiley and Sons, Inc., 1946).
[2] Bentley, H. R., and Whitehead, J. K., *J. Chem. Soc.* (in the press).
[3] Bentley, H. R., McDermott, E. E., Pace, J., Whitehead, J. K., and Moran, T., *Nature*, **165**, 150 (1950).

FORTHCOMING EVENTS

*(Meetings marked with an asterisk * are open to the public)*

Tuesday, May 9

MATHEMATICAL INSTITUTE (at 16 Chambers Street, Edinburgh), at 4 p.m.—Prof. M. Fréchet: "Abstract Distances".

PHYSICAL SOCIETY (at the Science Museum, Exhibition Road, London, S.W.7), at 4.30 p.m.—Annual General Meeting; at 5 p.m.—Scientific Papers.

ROYAL ANTHROPOLOGICAL INSTITUTE (at 21 Bedford Square, London, W.C.1), at 5 p.m.—Mr. A. J. Arkell: "The Excavation of a Neolithic Site at Esh Shaheinab in the Anglo-Egyptian Sudan".

ZOOLOGICAL SOCIETY OF LONDON (at the Zoological Gardens, Regent's Park, London, N.W.8), at 5 p.m.—Scientific Papers.

SOCIETY OF CHEMICAL INDUSTRY, CHEMICAL ENGINEERING GROUP (at the Geological Society, Burlington House, Piccadilly, London, W.1), at 5.30 p.m.—Dr. J. W. Barrett: "Silicones—their Production and Uses".

QUEKETT MICROSCOPICAL CLUB (at the Royal Society, Burlington House, Piccadilly, London, W.1), at 6 p.m.—Surgeon Lieut.-Comdr. Bunyan: "The Design of Microscopes".

SOCIETY OF INSTRUMENT TECHNOLOGY, NORTH-WEST SECTION (in the Reynolds Hall, College of Technology, Manchester), at 7.30 p.m.—Dr. N. S. Gregory: "Some Modern Aspects of Hygrometry".

ROYAL SOCIETY OF MEDICINE, PSYCHIATRY SECTION (at 1 Wimpole Street, London, W.1), at 8.30 p.m.—Prof. J. B. Rhine: "Psi Phenomena and Psychiatry". (Members of the British Psychological Society are invited to attend.)

Wednesday, May 10

INSTITUTE OF PETROLEUM (at Manson House, 26 Portland Place, London, W.1), at 5.30 p.m.—Mr. S. E. Coomber and Mr. E. N. Tiratsoo: "The Application of Radioactive Tracer Technique to the Study of the Movement of Oil in Sands".

INSTITUTION OF ELECTRICAL ENGINEERS (joint meeting of the RADIO and UTILIZATION SECTIONS, at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. B. Y. Mills: "A Million-Volt Resonant-Cavity X-Ray Tube".

PHYSICAL SOCIETY, LOW TEMPERATURE GROUP (in the Lecture Theatre, Science Museum, Exhibition Road, London, S.W.7), at 5.30 p.m.—Prof. Hausen: "Heat and Mass Transfer in Reflux Condensers".

INSTITUTE OF METALS (at the Royal Institution, Albemarle Street, London, W.1), at 6 p.m.—Dr. H. Roxbee Cox: "Industrial Gas Turbines" (Annual May Lecture).*

SOCIETY FOR PSYCHICAL RESEARCH (at Caxton Hall, Westminster, London, S.W.1), at 8.30 p.m.—Prof. J. B. Rhine: "Telepathy and Human Personality" (Tenth Myers Memorial Lecture).

Thursday, May 11

INSTITUTION OF WORKS MANAGERS, WEMBLEY SUB-BRANCH (at Hirst Hall, G.E.C. Estate, North Wembley), at 12.30 p.m.—Mr. A. L. Marden: "The Glass Industry".

MANCHESTER UNIVERSITY (in the Main Building, The University, Manchester), at 2 p.m.—Prof. M. Fréchet: "Differentials in General Analysis".

UNIVERSITY OF LONDON (in the Physiology Theatre, University College, Gower Street, London, W.C.1), at 4.45 p.m.—Dr. P. J. G. Mann: "Minor Elements in the Nutrition of Higher Plants".* (Further Lectures on May 18, 25 and June 1.)

LINNEAN SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—Dr. Hugh Scott, F.R.S.: "Journey to the Gughé Highlands (Southern Ethiopia)"; Dr. F. C. Fraser: "Diverticula of the Eustachian Tube of Cetaceans".

INSTITUTION OF ELECTRICAL ENGINEERS (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. P. Bauwens: "The Analysis of Action Potentials in Electromyography".

Friday, May 12

FARADAY SOCIETY, COLLOID AND BIOPHYSICS COMMITTEE (at the Institut Français, Queensberry Place, London, S.W.7), at 10.30 a.m. and 2 p.m.—Discussion on "The Physico-chemical Properties and Behaviour of Nucleic Acids".

MANCHESTER UNIVERSITY (in the Main Building, The University, Manchester), at 2 p.m.—Prof. M. Fréchet: "Random Abstract Elements".

CHEMICAL SOCIETY, BIRMINGHAM SECTION (joint meeting with the UNIVERSITY CHEMICAL SOCIETY, in the Main Chemistry Lecture Theatre, The University, Edgbaston, Birmingham), at 4.30 p.m.—Prof. C. A. Coulson: "The Shape of a Chemical Bond".

ROYAL ASTRONOMICAL SOCIETY (at Burlington House, Piccadilly, London, W.1), at 4.30 p.m.—Scientific Papers.

Friday, May 12—Sunday, May 14

BRITISH MYCOLOGICAL SOCIETY (with Headquarters at the Seymour Hotel, Totnes).—Thirty-first Spring Foray.

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

LECTURER with special responsibility in Zoology IN THE DEPARTMENT OF PHARMACY AND BIOLOGY—The Registrar, Central Technical College, Suffolk Street, Birmingham 1 (May 13).

JUNIOR PROFESSIONAL OFFICER, and a PLANT BREEDER, in the Tobacco Branch of the Department of Agriculture, Southern Rhodesia—The Secretary to the High Commissioner for Southern Rhodesia, 429 Strand, London, W.C.2 (May 15).

PROFESSOR OF CHEMICAL ENGINEERING AND TECHNOLOGY at Annamalai University, South India—The High Commissioner for India, India House, Aldwych, London, W.C.2, endorsed 'Application for Annamalai University' (May 15).

SENIOR LECTURER or LECTURER IN MECHANICAL ENGINEERING—The Registrar, University College, Southampton (May 15).

ASSISTANT LECTURER IN THE DEPARTMENT OF PURE MATHEMATICS—The Registrar, The University, Liverpool (May 20).

SALTERS' SCHOLARSHIP, and a SALTERS' FELLOWSHIP—The Clerk of the Salters' Company, Salters' Institute of Industrial Chemistry, 36 Portland Place, London, W.1 (May 22).

LECTURER IN PHILOSOPHY in University College, Dundee—The Secretary, The University, St. Andrews (May 30).

ADVISORY OFFICER (with a recognized diploma or degree in botany or agriculture)—The Secretary, Board of Greenkeeping Research, St. Ives Research Station, Bingley, Yorks (May 31).

BURSARIES IN CONCRETE TECHNOLOGY—The Deputy Registrar, City and Guilds' College, Exhibition Road, London, S.W.7 (May 31).

HEAD OF THE DEPARTMENT OF CHEMISTRY—The Director, Robert Gordon's Technical College, Aberdeen (May 31).

PROFESSOR OF CHEMISTRY AND HEAD OF THE CHEMISTRY AND PHARMACY DEPARTMENT—The Principal, College of Technology, Belfast (May 31).

PROFESSORS OF (*a*) MATHEMATICS, (*b*) ARCHÆOLOGY, (*c*) CLASSICS, (*d*) HISTORY, at the University College of the Gold Coast—The Secretary, Inter-University Council for Higher Education in the Colonies, 1 Gordon Square, London, W.C.1 (May 31).

SCHOLARSHIPS IN RUBBER TECHNOLOGY at the National College of Rubber Technology, Northern Polytechnic, Holloway, London, N.7—The Institution of the Rubber Industry, 12 Whitehall, London, S.W.1 (May 31).

ASSISTANT LECTURER IN THE DEPARTMENT OF OCEANOGRAPHY—The Registrar, The University, Liverpool (June 1).

ENGLISH ELECTRIC COMPANY BURSARS IN HYDRO-POWER ENGINEERING—The Deputy Registrar, City and Guilds' College, Exhibition Road, London, S.W.7 (June 1).

PROFESSOR OF CIVIL ENGINEERING at the University of Melbourne—The Secretary, Association of Universities of the British Commonwealth, 3 Gordon Square, London, W.C.1 (June 15).

PROFESSOR OF PATHOLOGY at the University of Melbourne—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (June 30).

PROFESSOR OF PHYSICS—The Registrar, Lucknow University, Lucknow, India (June 30).

RESEARCH STUDENTSHIPS IN SCIENCE—The Registrar, The University, Manchester 13 (July 1).

AGRICULTURAL ECONOMIST, and an ASSISTANT AGRICULTURAL ECONOMIST—The Registrar, University of Nottingham School of Agriculture, Nottingham.

DEVELOPMENT GROUP MANAGER to take charge of a Physics Research and Development Group, involving metrology, kinetic theory, properties of matter, fluid dynamics, heat, mass spectrometry and high vacuum technique, an ASSISTANT GROUP MANAGER to undertake mathematical research into atomic energy problems, and a WORKS PHYSICIST to undertake the duties outlined in Post 1, at the Atomic Energy Factory at Capenhurst, Chester—The Ministry of Supply, Division of Atomic Energy (Production), Risley, Warrington, Lancs.

LECTURER IN MATHEMATICS—The Clerk to the Governing Body, Battersea Polytechnic, Battersea, London, S.W.11.

LECTURER IN SCIENCE (graduate capable of teaching one or more of the following subjects: Zoology (Inter. and Final B.Sc.), Biochemistry, Nutrition, Microbiology, and Food Analysis, and a LECTURER (with special degree in Chemistry with a bias towards physical chemistry) IN THE DEPARTMENT OF CHEMISTRY—The Principal, Borough Polytechnic, Borough Road, London, S.E.1.

LECTURER IN TECHNICAL CHEMISTRY, with industrial and/or teaching experience, to specialize in Fuel Chemistry and Technology—The Secretary, Royal Technical College, Glasgow.

TYRE DESIGNERS and fully-trained RUBBER CHEMISTS—The Australian Scientific and Technical Mission, Australia House, Strand, London, W.C.2.

WOMEN (with honours degree in mathematics, physics or geography) to train as Meteorological Officers—The Director, Women's Royal Naval Service, Admiralty, London, S.W.1.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

British Museum (Natural History). Economic Series No. 4: Mosquitoes and their relation to Disease; their Life-History, Habits and Control. Fifth edition. Pp. 17. 6*d*. Economic Series No. 4*a*: British Mosquitoes and their Control. Third edition. Pp. 27. 6*d*. (London: British Museum (Natural History), 1949.) [301

Department of Scientific and Industrial Research: Road Laboratory. Road Research Technical Paper No. 15: Soil Survey Procedure. Pp. iv+32+4 plates. (London: H.M. Stationery Office, 1949.) 1*s*. net. [311

Colonial Office. Appointments in His Majesty's Colonial Service. Pp. 111. (London: H.M. Stationery Office, 1950.) 1*s*. net. [311

Parliamentary and Scientific Committee. Annual Report, 1949. Pp. 20. (London: Parliamentary and Scientific Committee, 1949.) 1*s*. [311

Philosophical Transactions of the Royal Society of London, Series A: Mathematical and Physical Sciences. No. 846, Vol. 242: The Lateral Instability of Yielded Mild Steel Beams of Rectangular Cross-Section. By B. G. Neal. Pp. 197–242. (London: Cambridge University Press, 1950.) 11*s*. [311